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SNAKE POISON.

THE most important class of chemical substances with which the physiologist has to deal is that of the proteids. Their importance arises from the fact that they form the most essential of the constituents of a diet, and the most constant and abundant of the materials obtainable from protoplasm and living structures generally. In spite of this, however, we know practically nothing of their chemical constitution. The physical properties of the proteids, their identification by chemical tests, their subdivision into classes according to their solubilities, and the products of their decomposition have all been pretty thoroughly studied; there also exist various theories of the way in which their molecule is built up; but there is nothing certain at present. One of the greatest feats in chemical science will be the synthesis of albumin; but until this is done our knowledge of that overwhelmingly important substance must remain nebulous, and advance in physiological chemistry be hindered.

Not the least strange of the many puzzling facts in connection with the proteids is that many of them are poisonous. The poisonous proteids are not distinguishable by any well-marked chemical or physical properties from the non-poisonous or food proteids. When the idea of a proteid poison was first mooted it was received with incredulity; and it was suggested that the real poison was something adherent to the proteid, and if the proteid had been

prepared in a pure condition it would be found to possess no toxic properties. This hypothesis may be correct, for the methods at present in vogue for obtaining pure proteids leave much to be desired. These methods, however, improve year by year; but as they improve, the toxic power of the poisonous members of the albuminous group does not diminish, and it appears more and more certain that it is the proteid itself which is the poisonous agent.

Proteid poisons have been obtained from both the vegetable and animal kingdoms. Thus among those obtained from plants, one may mention the proteids obtained from jequirity seeds, the proteid associated with or identical with the ferment papain of the papaw plant, and lupino-toxin from the yellow lupin.

The most important of the animal proteid poisons are snake poison; the proteids in the serum of the conger eel and other fish; and proteid poisons found in certain spiders.

Poisonous proteids are also formed during ordinary digestive processes in the alimentary canal of every one of us from the proteids taken in as food. The peptones and the proteoses or albumoses (intermediate products in the process of hydration of which the terminal product is peptone) are fairly powerful poisons. 0·3 gramme per kilogramme of body weight injected into the blood will kill a dog, producing a loss of coagulability of the blood, a fall of blood pressure, a stoppage of secretions, and ultimately death by cessation of respiratory activity. Normally animals are protected from this poison by the lining membrane of the alimentary canal, so that no proteose or peptone is found in blood or lymph even during the most active periods of digestion. The cells of this membrane possess many remarkable properties, but one of the most important is this power of regenerating albumin from peptone.

Allied to the albumoses of ordinary gastric activity are the similar products produced by bacteria. The way in which bacteria produce disease has long been a matter of dispute, but the problem appears to be approaching solution. Pathologists have at last turned their attention to the chemi-

cal side of the question, and shown that whereas in some cases the poisons produced by the growth of micro-organisms are alkaloidal in nature, in by far the greater number the toxic product is a proteid. The one which is best known, or at least attracted most attention, is the toxalbumose contained in Koch's tuberculin.

The foregoing list is far from complete, but one cannot conclude it without mentioning another class of proteid poisons: these are the nucleo-albumins obtainable by suitable methods from most of the cellular organs of the body. Originally discovered by Wooldridge they were named by him tissue-fibrinogens, because they possess the remarkable power of producing coagulation of the blood within the blood vessels of a living animal. A very small dose will kill a rabbit or a dog, and death is as a rule produced by extensive clotting within the vessels, especially in the veins. Under certain conditions, however, especially in the dog, they produce the opposite result, namely a loss of coagulability similar to that produced by peptone. Wooldridge termed this the "negative phase of coagulation".

A practical outcome of all this work is the discovery of alexines or protective proteids. These appear to belong to the nucleo-albumin class also. In small doses they confer immunity on animals to larger doses of similar poisons, and thus the long-hidden secret of the *modus operandi* of vaccination and other forms of protective inoculation is at last beginning to be unravelled.

The limits of the present article would, however, be far exceeded if one were to take up fully all the points hitherto alluded to, and follow them into the various scientific and practical channels into which they have led. I therefore propose in the remainder of this paper to consider in detail one class only of the poisonous proteids: this is one which to the Englishman is of theoretical interest only, but to many thousands of our fellow-creatures it possesses a deep practical importance also: the poisons in question are those which are secreted by snakes.

In so doing I shall allude chiefly to the most recent work on the subject by Dr. C. J. Martin and Mr. J. M'G.

Smith of Sydney, who have obtained results in the case of Australian snakes which corroborate those obtained by previous observers with the cobra and other venomous serpents.[1] Many of the paragraphs that follow are quotations from these papers.

A complete investigation into the subject of snake poison must attempt to answer three questions:—

1. What is the poison?
2. What is its physiological action?
3. How can one best prevent or counteract this action?

The majority of previous workers have begun at the wrong end; for out of about four hundred references consulted, over three hundred are to papers in which the authors answer to their own satisfaction the third question, and describe the beneficial results following the administration of some such potent drugs as ash tea or human saliva, and the utter and entire futility of the whisky or other treatment.

Martin has adopted another method, and his papers relate to the first two questions only. He is to be congratulated on his results, especially as the investigation was fraught with difficulties. It was impossible to procure the services of a professional snake catcher, and so it was necessary for him to do all the work himself. As he puts it, it was also necessary to overcome that dislike and dread of the serpent which is instilled into the youthful intelligence at an early age in every Christian land.

The method of obtaining the poison was an ingenious modification of that adopted by the Indian snake men. The yield of poison per bite was very small, and so considerable time and patience were consumed in getting enough material to work with.

The small quantity secreted is apparently amply atoned for by quality, the minimal fatal dose per pound weight being considerably less than that given by the Indian Snake Commission for the cobra. Some idea of this virulence

[1] The Venom of the Australian Black Snake (Pseudechis porphyriacus), by C. J. Martin and J. M'Garvie Smith, *Proc. Roy. Soc. N. S. Wales*, Aug. 3, 1892; and *Journal of Physiology*, vol. xv. (1893), p. 380.

may be gathered from the fact that one-thousandth part of a grain invariably kills a rabbit of five pounds weight in about a hundred seconds.

This extraordinary toxicity becomes more astounding still when we consider that the poison is a proteid undistinguishable by chemical methods from those daily used as food by all of us.

The question of chemical constitution we take next. The first investigation into the chemistry of the snake poison of any importance was by Prince Lucien Buonaparte on the poison of an adder in 1843. He found that the activity of the poison was associated with that portion precipitated by alcohol; and he gave the name "viperine" to this precipitate. Dr. Weir Mitchell next turned his attention to the subject about 1860; and he is essentially the founder of our present knowledge concerning snake poison. Crude as were the methods of animal chemistry in his day, they nevertheless led him to the right conclusion that the toxic principle of the venom is albuminoid in nature. He termed it "croatalin" in the case of the rattlesnake. From that time till 1886, in conjunction with Reichert, he continued his work, and confirmed his general conclusion in the case of other North American snakes. About 1871 the Indian snakes received their share of attention; and the names of Sir Joseph Fayrer and Dr. Lauder Brunton are associated with valuable researches concerning the venom of the cobra, kraits and the Indian viper.

In 1883 Wall, in 1886 Wolfenden, and in 1893 Kanthack published most instructive contributions to our knowledge of cobra venom; the improved methods of chemical physiology enabling them not only to identify the poison as a proteid, but to show that the variety of proteid present is an albumose. Two observers only of importance have described poisons other than proteid in snake venom: one of these was Gautier, who regarded the venomous principle as alkaloidal; and the other, Winter Blyth, who gave the name "cobric acid" to a highly poisonous crystalline substance he claimed to have separated from cobra venom. Recent work has failed to substantiate these results, and alkaloids when

present at all (and they are generally absent) are non-poisonous ones.

In the researches on the venom of the Australian black snake, Martin and Smith found it necessary to exclude various classes of poisons, as well as to determine positively the nature of the venom. They excluded in the first place by appropriate experiments the presence of micro-organisms, ferments, alkaloids, ptomaines, and crystalline acids.[1] In the second place they showed that the poison was a proteid. The methods for the separation of proteids from one another are highly technical. It will therefore be sufficient to say that the manipulations were of the most recent and perfect kind, and pass to the results obtained. In the proteid mixture three proteids were obtained: one an albumin, and the other two albumoses. The albumin is not virulent, but the two albumoses (corresponding to proto- and hetero-albumoses of Kühne) are extremely poisonous. They each have the same physiological action, and this is the same as that produced by the venom itself. The venom can be momentarily boiled without impairing its activity, but prolonged boiling for days destroys its virulence.

The action of the poison may be described under two heads, (1) local and (2) general effects. The most marked of the local effects is œdema; the general symptoms consist of twitching and convulsions in non-lethal doses. A fatal dose kills within a few seconds or minutes. There is also a peculiar effect on the blood, which I propose to deal with more in detail immediately.

The conception put forward of the formation of these albumoses is the following:—

The cells of the venom-gland by a vital process exercise a hydrating influence on the albumins supplied to them by the blood, the results of which influence are the albumoses found in the venom. The difference between this process and digestion by pepsin or by anthrax bacilli is that the hydration stops short at the albumose stage, and is not continued so as to form peptone or simpler nitrogenous pro-

[1] A questionable trace of an organic acid found did not possess toxic properties.

ducts like leucine, tyrosine or alkaloids. Gland epithelium is certainly capable of exercising such a hydrating influence; the conversion of glycogen into sugar by the liver cells is one of the best known examples.

The following table, somewhat altered from Dr. Sidney Martin's Goulstonian lectures to the Royal College of Physicians, London (1892), illustrates the analogy between various hydration processes, proteid being in all cases the material acted on.

Primary Agents.	Ferment.	Products.	
		Albuminous.	Nitrogenous, but not Albuminous.
1. Epithelial cell of gastric glands.	Pepsin.	Albumoses. Peptone.	Pepto-toxine. (?)
2. Epithelial cell of pancreas.	Trypsin.	Globular-like substance. Albumoses. Peptone.	Leucine, tyrosine, asparticacid, ammonia.
3. Bacillus anthracis.	None yet found.	Albumoses. Peptone.	Leucine, tyrosine, and an anthrax alkaloid.
4. Bacillus diphtheriæ.	Ferment not yet named.	Albumoses.	Organic acid of doubtful nature.
5. Epithelial cell of snake's venom-gland.	None yet found.	Albumoses.	Trace of organic acid. (?)

I have left to the last the effect of snake venom on the blood, because it opens up other questions, and has no particular bearing on the general aspect of the subject of proteid poisons. Fontana, more than a hundred years ago, noticed that the blood remained fluid in animals dead of viper bite, and Brainard, writing forty years back, states that when death occurred *immediately* in animals bitten by rattlesnakes the blood was found at the *post-mortem* examination to be clotted; but if some time elapsed before the animal succumbed, the blood remained fluid in the vessels. The continued fluidity of the blood has since then been noted by numerous observers in the case of various snakes.

Martin made most of his observations on dogs, but obtained confirmatory results on other animals (cats and rabbits). He found that different doses produced different results. Immediately after the introduction of the venom the coagulability of the blood increases; and this increase in the case of moderate or large doses (more than 0·0001 gramme per kilo. of body weight) culminates in intravascular clotting of greater or less extent. The injection of smaller doses produces a transient phase of increased coagulability, but after two minutes this is succeeded by a "negative phase"; the blood drawn either fails to clot at all, or does so only after the lapse of several hours. The thrombosis occurs more readily in venous than arterial blood, and is frequently confined to the portal area.

These results show a great resemblance between the action of the venom and that of tissue-fibrogen or nucleo-albumin. The effect of diminished coagulability is not unexpected, seeing that the principal substance in the venom is albumose. But the minuteness of the dose necessary is very striking and distinctive.

The question arises, does the poison contain nucleo-albumin? A nucleo-albumin is a proteid united to a substance rich in phosphorus, called nuclein. It can be detected by the fact that artificial gastric digestion dissolves the proteid and leaves the nuclein as an insoluble residue. This residue must then be examined for phosphorus. Snake venom contains no nucleo-albumin; and its action not only opens up a novel aspect of the subject of snake poisoning, but also sheds light on the vexed problem of blood coagulation.

The smallness of the dose suggests that the injected material does not contribute itself to fibrin formation. Probably it acts by producing disintegration of the cells in proximity to the blood stream, such as the endothelial cells lining the vascular system. If it thus liberates nucleo-albumin from these the conditions would be practically the same as if this toxic agent were injected from without. The venom *is* capable of playing havoc with these cells. This was originally shown by Weir Mitchell and Reichert. These

authors moistened the mesentery of a cat with a solution of rattlesnake venom, and observed under the microscope the rapid formation of extensive capillary hæmorrhages. Martin repeated these experiments, using black snake venom; and although the action of this poison is less rapid than was the case in Mitchell's experiments, the results were identical.

Whether the venom causes any destruction of the white blood corpuscles is doubtful. These are massed together in such a way that their enumeration becomes a difficult matter. The plasma is stained with hæmoglobin, indicating that a slight solvent action on the red corpuscles has taken place. This, however, is not a distinctive action of snake venom. It is, moreover, well known that substances like distilled water which produce extensive disintegration of blood corpuscles within the blood stream never produce thrombosis; so that, even if the venom produces a disappearance of the leucocytes, that would in itself be insufficient to cause intravascular coagulation.

From this summary of the subject of snake poison one sees how much of interest exists in such researches. They open up fresh questions in wide and important general subjects, two of which, namely, blood coagulation and the poisonous nature of certain proteids, it has been the object of this paper specially to emphasise.

W. D. HALLIBURTON.

ALGÆ AS ROCK-BUILDING ORGANISMS.

FROM the point of view of a botanist the phycological records of the rocks are exceedingly disappointing. The Lower Palæozoic strata have afforded such genera as *Nematophycus* and *Pachytheca*, which may perhaps be regarded as extinct forms of algæ, but cannot as yet be assigned to any definite position in that class of plants. The tissues of these two genera have been described by various observers, and botanists have exercised considerable ingenuity in their speculations as to the exact position of both fossils in the vegetable kingdom.

Apart from *Nematophycus* and *Pachytheca* our knowledge, such as it is, of fossil algæ is limited to structureless specimens of various forms and sizes, which have been referred to algal genera on the evidence of any superficial resemblance to the bodies of living forms.

The calcareous algæ afford a notable exception to the general rule as regards the unsatisfactory state of fossil phycology. Their hard parts have left well-preserved remains in rocks of different ages, and have supplied trustworthy data for the study of extinct forms. In addition to the calcareous algæ, and a very few isolated examples of non-calcareous genera, there are but few records which can be utilised in phylogenetic studies. When we approach the subject of fossil algæ from a geological standpoint, we are confronted with a mass of facts, collected for the most part in recent years, which reveal the great importance of algal remains as rock-building agents. It would be superfluous to draw attention to such diatomaceous deposits as occur in the Cretaceous and Tertiary formations, or to describe the various conditions under which diatoms are now building up siliceous rocks. If we accept Heer's genus *Bactryllium* as an unusual type of diatom we may extend the geological history of these rock-builders to Keuper and Muschelkalk times, but from older strata trustworthy records have still to be sought for.

At the present day we find such genera as *Lithothamnion*, *Lithophyllum* and others taking a more or less prominent part in rock construction; in some cases adding their calcareous skeletons to the accumulating masses of coral reefs; under other conditions spreading over the ocean floor in high northern latitudes.[1] The thick masses of limestone of various geological ages containing numberless specimens of calcareous Florideæ testify to the widespread occurrence of the same and similar forms in the ancient seas. Other strata are obviously built up very largely of calcareous algæ of a simpler type, and reveal the past history of numerous genera which must be placed in the same group with *Cymopolia* and other recent forms. Not only do many rocks supply clear and undoubted proof of their partial or complete development from the accumulation of calcareous plants, but in many cases there are striking facts to hand which point to a phytogenetic origin for strata without any present traces of organic structure.

OOLITIC ROCKS AND ALGÆ.

Oolitic structure characterises many rocks of Palæozoic, possibly also pre-Palæozoic, Mesozoic and Tertiary age, and is of common occurrence in recent calcareous deposits. The best known examples of oolitic rocks are those which form part of the Oolitic series in the Jurassic formation. A typical oolite consists of "rounded particles varying in size from a pin's head to a pea,"[2] with usually some foreign body as a central nucleus round which the calcareous substance has been deposited. The microscopic structure and mode of origin of oolitic sediments were made the subject of examination by Dr. Sorby some years ago, and it was generally agreed that the carbonate of lime of each grain had been deposited layer by layer round a grain of sand, shell fragment or other nucleus as it was carried to and fro in an eddying current. At all events the formation of oolitic grains was referred to some essentially inorganic process.

[1] Kjellman, p. 66. [2] Green, p. 281.

The author of a recent paper on oolitic structure concludes with the following words: "In concluding this paper I should like to say, first, that it has been my object to produce evidence that oolitic structure is not always of concretionary origin. That it is all organic I am not prepared to maintain, but it may be."[1] The change of view thus expressed by Wethered is the outcome of a more intimate acquaintance with the minute structure of oolitic grains. In sections of oolitic rocks from various Palæozoic and Mesozoic horizons it has been conclusively demonstrated that a small tubular structure is of very frequent occurrence in the individual calcareous grains. To this tubular fossil has been assigned the name *Girvanella*. The name was chosen from the discovery of this structure in Ordovician rocks in the Girvan district of Scotland. Nicholson and Etheridge have given the following diagnosis of the genus: "Microscopic tubuli, with arenaceous or calcareous (?) walls, flexuous or contorted, circular in section, forming loosely compacted masses. The tubes, apparently simple cylinders, without perforations in their sides, and destitute of internal partitions or other structures of a similar kind."[2] Without expressing themselves very decidedly as to the systematic position of the genus the authors are inclined to regard it as a rhizopod. Since its discovery in the Girvan rocks *Girvanella* has been repeatedly found by other observers in strata of different ages. The careful researches of Wethered have been especially important in drawing attention to the widespread occurrence of this problematical organism. Possibly it may eventually find a definite place among fossil algæ, but at present it is safer merely to suggest such a systematic position rather than to attempt a more precise determination. The discovery of this new structure in oolitic grains has naturally called forth numerous and diverse expressions of opinion as to its probable nature, but it is unnecessary to attempt a detailed examination of such speculations.

In an interesting contribution from Prof. Nicholson,

[1] Wethered (2), p. 281. [2] Nicholson and Etheridge, p. 23.

"On certain anomalous organisms which are concerned in the formation of some of the Palæozoic limestones,"[1] the author adheres to his original opinion as to the affinities of *Girvanella.* Two figures accompanying his paper afford a good idea of the nature of this doubtful genus: in one a section of the Craighead (Ayrshire) limestone is shown to contain several small rounded or irregular nodules suggestive of oolitic structure; the second figure exhibits the characteristic tubular structure of the *Girvanella* clusters. Nicholson also notes the occurrence of the same fossil in certain Lower Palæozoic rocks of North America. In 1889 Wethered discovered the same tubular structure in Jurassic pisolites;[2] he examined microscopic sections of Coralline Oolite from the neighbourhood of Weymouth and of Pea Grit from the northern Cotteswolds. In both cases the calcareous spherules were found to have a central nucleus surrounded by rudely concentric layers of innumerable minute tubuli exhibiting "vermiform twistings and turnings". Such a microscopic structure in the pisolite grains was naturally regarded as entirely opposed to the ordinary view as to the concretionary origin of oolitic structure. Each granule was shown to have resulted from the growth of some organism round a central nucleus; the organism itself Wethered referred to Nicholson's genus *Girvanella.* By his subsequent investigations Wethered has considerably extended the range of this fossil, and further evidence has been accumulated as to its frequent connection with oolitic structure. An oolitic structure is fairly common in the Carboniferous limestone of Gloucestershire, and in some localities *Girvanella* has been detected in the individual spherules, the oolitic grains occasionally having a nucleus in the form of a foraminiferal shell fragment, but more frequently in these older rocks the central calcareous fragment has been crystallised as calcite.

The tubules of this Carboniferous species, *Girvanella Ducii*, are wrapped round a central nucleus, and impart to the spherules an appearance suggestive of concentric layers

[1] Nicholson, p. 23, fig. 5, A and B. [2] Wethered (1).

of growth. In another species from the same rocks, *G. incrustans*,[1] the narrow tubules form a crust round a calcite nucleus. The Jurassic Oolites have afforded such species as *G. pisolitica*, *G. intermedia* and *G. minuta*. In all these cases the organism consists of tortuous and aggregated tubules exhibiting certain variations in size and manner of arrangement. The frequent association of this structure with oolitic grains leads Wethered to the conclusion that the spherules of such rocks must be referred rather to an organic than to a concretionary origin.

Dr. Hinde has called attention to the occurrence of *Girvanella* in Ordovician rocks in the province of Quebec, and to the discovery by Bornemann of similar structures in Cambrian rocks of Sardinia.[2] Bornemann has instituted a new genus *Siphonema*[3] for certain incrusting calcareous algæ consisting of minute tubular cells. He compares the Sardinian genus to the recent *Scytonomaceæ*, and expresses the opinion that in all probability many of the oolites from various geological horizons will be found to supply similar proof of their organic origin. The nature of these Sardinian fossils and their concentric disposition round fragments of shells, crinoids or corals are very like that of *Girvanella*, and there can be little doubt that Bornemann's genus is the same as that previously established by Nicholson and Etheridge.

The species of *Siphonema*, *S. incrustans*, which occurs in the Sardinian rocks has also been found by Bornemann in a limestone boulder which probably came from Baltic rocks of Silurian age. Additional information has been collected by Wethered as the result of his microscopical researches on the structure of Oolitic rocks from the Cotteswold Hills.[4] In a still more recent communication to the Geological Society the same author refers to the occurrence of *Girvanella* tubules in Devonian limestones from South Devon;[5] and on another occasion the existence of *G. pro-*

[1] Wethered (2), p. 280, pl. xi., figs. 1 *a*, *b*.
[2] Wethered (2), p. 282.
[3] Bornemann (1), pl. ii.
[4] Wethered (3).
[5] Wethered (4).

blematica, associated with other species of the same genus, is recorded in the Wenlock limestones of May Hill.

Wethered considers that the most interesting result of his microscopical examination of the Wenlock limestone has been the discovery of "new and interesting forms of *Girvanella*, and the fact that this organism has taken so important a part in building up limestones".[1] Aggregations of *G. problematica* were met with in rocks of the same age near West Malvern and in sections of limestone from Ledbury. In summarising his results Wethered makes certain suggestive remarks as to the method of formation of some of the Wenlock beds in which the tubular organism is so abundant; he writes as follows: "Fragments of organisms were deposited on the sea-floor, around these the *Girvanella* tubules collected, sometimes entirely enclosing a fragment with a crust of tubules, and thus giving rise to a spherule or granule, the determination of which depended on the shape of the fragment enclosed".[2]

Without attempting any complete discussion of the references by other writers to this important and almost ubiquitous fossil, we must note the fact, recently pointed out by Rothpletz,[3] that Nicholson has declared himself favourable to the inclusion of *Girvanella* among (calcareous) algæ.

It has been suggested by Clement Reid[4] that possibly the calcareous tubules of *Girvanella* may be looked upon as cylindrical sheaths in which small filamentous algæ became enclosed. If such an explanation be accepted we can hardly speak of the genus as a true calcareous alga, but as an alga which was able to bring about a deposition of carbonate of lime outside itself, and not actually in its own cell walls.

Another author, Dr. Brown,[5] has recently lent his support to the opinion, previously expressed by Rothpletz and others, that *Girvanella* must probably be placed among calcareous algæ in the family *Siphoneæ*.

[1] Wethered (5), p. 239. [2] Wethered (5), p. 246.
[3] Rothpletz (1), p. 302. [4] Wethered (5), p. 248.
[5] Brown, p. 203.

How far do other observations on the origin of oolitic grains support the contention that *Girvanella* has in many instances been instrumental in producing such rock structure? On the shores of the Great Salt Lake of North America there are accumulations of snow-white calcareous grains which the waves have washed up on the beach; these minute spherules correspond in the arrangement of the carbonate of lime, and in external form, to typical oolitic grains. The nature of this modern oolitic material has recently been made the subject of examination by Rothpletz.[1] On dissolving the carbonate of lime he finds that each grain shows a residue of algal cells, some of which he compares to *Glœocapsa*. Cells of this genus and of *Glœotheca*, surrounded by the usual gelatinous sheaths, occur as a coating to the calcareous spherules whilst still in the waters of the lake. In all probability the Salt Lake oolite is a product of algal life. Rothpletz's researches in oolitic structure lead him to the conclusion that at least the majority of marine calcareous oolites, which exhibit a regular zonal and radial structure, must be looked upon as the results of algal life. Another example of oolitic structure closely connected with the growth of algæ may be quoted from an important work by Walther. This author describes oolitic grains from the shores of the Red Sea which have a central nucleus of quartz, felspar or garnet surrounded by carbonate of lime enclosing algal cells and branched tubular structures.[2]

Finally, Bleicher[3] has discovered organic matter in Jurassic oolitic grains from Lorraine, and in some cases this takes the form of a tubular network of algal-like filaments. Here, again, we have an organism, and presumably an alga, entering into the composition and concerned in the formation of oolitic grains.

It appears from the researches of Rothpletz that in some cases another form of alga closely allied to *Girvanella* has played an important part in the production of oolitic structure; this is the genus *Sphærocodium*, which he

[1] Rothpletz (2). [2] Walther (2), p. 482.
[3] Bleicher (1) and (2).

describes from the Upper Alpine Trias and places in the family *Codiaceæ*.[1] It occurs in small spherical or oval aggregations which were formerly regarded as concretionary oolitic spherules. In the Raibler and St. Cassian beds there occur bands of limestone to a large extent built up of these *Sphærocodium* spherules; Rothpletz figures and describes this alga as *S. Bornemanni*.

According to Bornemann[2] a fossil genus of the *Rivulariaceæ* has also been responsible for oolitic structure in some cases; he institutes the genus *Zonatrichites* for this form, and gives the following definition of it: "A fossil calcareous alga with radially arranged filaments, forming hemispherical or kidney-shaped layers, growing on or enclosing other bodies. Parallel or concentric zones are seen in cross section, formed by the periodic growth of the alga, the older layers serving as foundations on which the younger filaments grow in radially arranged groups."

In an appendix to Bornemann's paper the question of oolitic structure is dealt with, and descriptions are given of microscopic preparations of oolitic rocks showing the nature of the algal elements in the individual grains.

In his paper on rock-building algæ in the Swiss Alps, Früh[3] calls attention to the association of *Zonatrichites* with oolitic structure.

As another example of recent algæ as oolite-builders, reference should be made to Weed's description of the travertine deposits at the Mammoth hot springs in the Yellowstone Park.[4]

Enough evidence has been adduced, without citing further examples, to show that there is reason to believe that algæ have a much greater claim to the attention of geologists as possible agents of rock construction than has generally been admitted.

[1] Rothpletz (1), pl. xv., figs. 5 and 6, etc.
[2] Bornemann (2), pls. v. and vi.
[3] Früh.
[4] Weed, p. 643.

CALCAREOUS ALGÆ OF THE CLASS FLORIDEÆ.

The Miocene Leithakalk of the Vienna Basin has long been quoted in text-books as a rock largely composed of the genus *Lithothamnion*. This characteristic fossil was originally described by Reuss as *Nullipora ramosissima* and placed by him in the category of fossil animals. The genus *Nullipora* has been used in an extremely wide sense, embracing organisms which are for the most part members of the vegetable kingdom, but including others which must still be retained as animals. An important contribution to the study of the Leithakalk appeared in 1858 by Unger.[1] He refers to the discovery by Philippi, in 1857, that several of the so-called Nullipores had been erroneously included in the animal kingdom, and goes on to describe in detail the structure of *Nullipora ramosissima* from the Vienna limestones. Unger gives descriptions and figures of the genera *Lithothamnion* and *Lithophyllum*, and recognises the important *rôle* of the fossil forms of these and similar algæ in the formation of limestones.

The microscopic examination of the Leithakalk has been considerably extended by Gümbel, who gives a number of interesting facts as to the frequent occurrence of calcareous algæ in limestones of different ages and localities.[2]

Specimens of *Lithothamnion* are described from Jurassic rocks near Neukirchen, the Maestricht chalk, Pisolites of the Paris Basin, etc.

With a view to throwing fresh light on the geological action of calcareous rock-forming algæ, Walther availed himself of the facilities afforded by the Biological Station at Naples to make a special study of living rock-building plants. He examined the Secca di Chiaja, the Secca della Gajola and the Secca di Penta palumno, and investigated the formation of calcareous deposits at present in operation in those submarine plateaux.

The Secca della Gajola is described as a typical example of a recent algal deposit; it is situated about one kilo. from the coast and 30 m. below the surface of the water.[3]

[1] Unger. [2] Gümbel. [3] Walther (1).

Numberless nodules of *Lithothamnion ramulosum* and *L. racemus* are brought up in the dredgings from this calcareous bank; also foraminifera, gasteropods, diatoms and other organisms. By the action of percolating water the *Lithothamnion* structure is gradually obliterated, and the calcareous mass becomes a structureless limestone.

Walther applies his knowledge of this recent algal deposit to the examination of a Tertiary "Nulliporenkalk" near Syracuse. In many parts of this formation there occur well-preserved specimens of *Lithothamnion*, but in others a gradual obliteration is observed of all plant structure until the rock becomes entirely structureless. A similar instance of structureless limestone is described from the Lias of Todten Gebirges; the strata consist of coral rock, detrital calcareous deposits, and, associated with these, masses of limestone in which microscopic examination fails to detect either vegetable or animal structure. These structureless beds are considered to have been *Lithothamnion* banks from which percolating water has removed all trace of algal cells. It is suggested that the infiltrating water was supplied by the *Lithothamnion* thallus with the necessary amount of carbonic acid, and was thus enabled to remove all direct evidence of the existence of calcareous algæ. In connection with this solvent power of the water Walther asks the question: "What becomes of the plant cellulose in the process of fossilisation?" An instructive comparison is made between the chemical composition of compact *Lithothamnion* masses from the Secca di Penta palumno and the Tertiary *Lithothamnion* limestones in the neighbourhood of Syracuse; in the former the $CaCo_3$ reaches 86°/₀, and the organic substance 5°/₀; in the latter the $CaCo_3$ reaches 98°/₀, and organic substance 0·28°/₀. The organic substance of the algæ became chemically altered in the Syracuse beds, and in the course of such changes carbonic acid was evolved; this was readily taken up in solution by percolating water, which was thus supplied with the means of obliterating all traces of *Lithothamnion* structure.

Thus it is shown that in masses of calcareous algal remains there is an "endogenous source" of carbonic acid

which may frequently result in the removal of all signs of phytogenetic origin. On the other hand, in many calcareous beds the percolating waters have not found the same amount of carbonic acid, and their solvent power has not been sufficient to effect the destruction of the organic remains from which the strata have been formed. If the calcareous deposits are protected from the circulation of carbonated water by overlying impervious beds, the organic structures would not be removed. It would seem, therefore, that under certain circumstances, in which calcareous deposits are freely exposed to infiltrating water, there is a much greater probability of all structure being removed in the case of those formed from calcareous algæ than in deposits which are not of phytogenetic origin. Walther's researches are of extreme importance from a geological point of view; they at least show that the study of living calcareous algæ may possibly lead the petrologist to approach from a new standpoint the difficult question of the genesis of structureless limestones.

In two recent numbers of the *Geological Magazine* Brown [1] has given some account of another calcareous organism, *Solenopora*, which his investigations compel us to annex as a member of the *Florideæ*. It has a wide geographical distribution, and its geological range extends from Ordovician to Jurassic times. The author describes several species from various districts and horizons, and concludes his remarks with a discussion of the botanical position of the genus. He compares it with *Lithothamnion*, *Corallina*, and other calcareous genera: "the form of the cells and cell walls, the method of increase, and the arrangement of the tissue cells in the various species of *Solenopora* bear strong evidence of relationship between that genus and the calcareous algæ". Additional evidence of taxonomic value is derived from the occurrence of reproductive organs in some few specimens. There can be little doubt that *Solenopora* should be included among the rapidly growing list of fossil calcareous algæ which must claim the atten-

[1] Brown, p. 200, pl. v.

tion of phycologists, as well as increased recognition on the part of geologists.

A recent discovery of *Lithothamnion* in a cutting at the mouth of the River Liffey deserves a passing notice. In deepening the extremity of a well in 1871-72 several shells which had been dug up in the process of excavation were found to be grown over with some calcareous organisms. These encrusting growths were determined to be species of *Lithothamnion*, very like *L. polymorpha* and *L. fasciculatum*, both of which are still living in the Bay of Dublin, but no longer exist in the Liffey.[1]

ALGÆ AND HOT-SPRING DEPOSITS.

In an exhaustive monograph on this subject Weed expresses his belief that "the geological work of plant life has not been generally recognised, partly because it is less conspicuous, and partly because the absence of organic remains in many deposits formed in this way has prevented a recognition of the true origin of the rocks".[2] The able account by this geologist of the Yellowstone Park hot springs should have the effect of raising the importance of the lower forms of plant life as geological agents.

Records of algæ in the waters of hot springs have been supplied by various observers, and the power of such plants to withstand high temperatures has been long known. Weed has done good service in calling attention to the consequences of such algal life in thermal waters. The exact nature of the vegetation in the Yellowstone Park springs is often difficult to determine, the plants being frequently encrusted and hidden by such substances as sulphur, sulphate of calcium, oxide of iron, etc. In the district of the Mammoth hot springs, extensive masses of travertine or calcareous tufa have been gradually built up. The springs themselves vary in temperature from 80° to 165° F., and all have afforded numerous examples of actively living plants. The growths vary considerably in character and colour according to the temperature of the water; in rapidly flow-

[1] O'Reilly.

[2] Weed, p. 619.

ing currents the plants assume a filamentous form, in quieter pools they are united in membrane-like sheets. Not infrequently the filaments are embedded in travertine with the tips alone projecting. The withdrawal by the plants of carbonic acid from the water causes a separation and precipitation of carbonate of lime, which gradually gives rise to fan-shaped or terraced masses of calcareous rock. About many of the springs there occur masses of gelatinous algal growths, consisting of successive membranous sheets; in these are embedded minute gritty particles which on further development become aggregated together in the form of small grains of carbonate of lime. These spherules afford another example of algal oolite. In the older layers the oolitic structure is masked by the cementing together of the separate pellets, and thus the gelatinous masses gradually pass into firm and more or less compact travertine. The travertine of the Mammoth hot springs which has not been the result of such organic agencies forms but a small part of the deposits. In describing the more fibrous part of the calcareous masses Weed refers to its obvious organic origin, and speaks of the fine algal threads which have been instrumental in its formation, "as if a skein of silk floating in the shifting currents of a stream were suddenly turned to snowy travertine".

The following causes are mentioned as having been concerned in the production of travertine deposits: Relief of pressure, diffusion of the carbonic acid by exposure to the atmosphere, evaporation, heating and the influence of plant life.

Turning to the siliceous springs of the Yellowstone Park region the same observer finds that much of the siliceous sinter has been formed by the growth of such plants as algæ and mosses. The brilliant tints of red, yellow and green are all produced by such living plants as have the power of separating the substance of the sinter from the hot siliceous waters.

In some places the algæ occur in the form of tough leathery sheets of gelatinous material, in others they appear as delicate skeins of white filaments. As in the

calcareous springs, here too a gradual transition may be traced from the living gelatinous growths to hard and compact sinter. *Calothrix gypsophila*, *Leptothrix* and other genera are recorded from these sinter-forming springs; such plants are spoken of as algæ, but probably they would be more fitly described as bacteria. Some forms of the siliceous sinter appear to have been mainly formed by a species of moss, *Hypnum aduncum* var. *grasilesiens* Br. and Sch.

The causes which have operated in the formation of sinter are summarised as follows: Relief of pressure, cooling, chemical reaction and evaporation.

This subject of sinter-building by low forms of plants was discussed at some length in an important communication from Prof. Cohn[1] in 1862, in which he demonstrated the importance of such forms in the deposition of the Carlsbad "Sprudelstein".

When we take account of the occurrence of algæ and bacteria in the hot springs of New Zealand, Iceland, India, the Azores, Malay Archipelago, Japan, America and other places, it must be admitted that Weed had good grounds for asserting that the geological work of the lower forms of plant life has not been generally appreciated.

SIPHONEÆ VERTICILLATÆ.

Did space permit some account might be given of the numerous calcareous algæ in the Triassic limestones of the Southern Alps. Such genera as *Dactylopora*, *Haploporella*,[2] *Gyroporella*, *Uteria* and many others were originally described as foraminifera by Carpenter, Gümbel and other writers. Gümbel included in the family *Dactyloyideæ*[3] a rich variety of forms possessing calcareous skeletons, and his papers contain valuable information as to the struc-

[1] Cohn.

[2] It should be noted that Rothpletz has recently pointed out that *Haploporeila fasciculata* Gümbel is not an alga, but consists of echinoid spines (*Neues Jahrb.*, vol. i., 1891, p. 285).

[3] Gümbel, p. 253.

ture and distribution of living and fossil species. He refers to the occurrence of several genera and species from the Tertiary Paris Basin, the Trias of the Alps and Silesia, and from other geological formations. A few years subsequent to the appearance of Gümbel's monograph, Munier-Chalmas[1] made out a clear case for the removal of these so-called foraminifera to a new class of calcareous algæ to which he gave the name of *Siphoneæ Verticillatæ*. In this class he included fifty genera ranging from the Trias to the present day, all characterised by consisting of a simple or dichotomously branched frond with a tubular unicellular axis giving off verticils of branches. A useful account of these important plants will be found in Solms-Laubach's *Fossil Botany*;[2] his critical remarks carry the weight of an authority on recent calcareous algæ.

The recent researches of Bertrand and Renault point to the operation of simple forms of algal life in the production of such carbonaceous deposits as the boghead of Autun, Scotland and other districts; reference was made to the work of these authors in the first number of "SCIENCE PROGRESS".[3]

NOTE.—Since writing this article my attention has been drawn to a recent paper by Wöhrmann (Alpine and Ausseralpine Trias) in the *Neues Jahrbuch*, bd. ii., heft 1, 1894, p. 1. The author lays stress on the important share which algæ have undoubtedly had in the formation of thick masses of limestone and dolomite rocks; the Codiaceæ and Siphoneæ are the families which have been chiefly concerned in the building of these Triassic strata.

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[1] Munier-Chalmas. [2] Solms-Laubach, p. 37.
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A. C. SEWARD.

THE BIOLOGICAL CHARACTERS OF BACILLUS TYPHOSUS (EBERTH) AND BACTERIUM COLI COMMUNE (ESCHERICH).

BACTERIOLOGY offers so many problems for the consideration of investigators in allied sciences that the remarkable growth of a considerable literature in connection with this subject is easily understood. Questions which ten years ago were prominent, such as the monomorphic or pleomorphic characters of bacteria, have to-day an interest which is secondary to their physiological and pathological behaviour. The study of any single micro-organism tends to confirm the belief in the flexibility of form which many saprophytic and parasitic bacteria may exhibit, and consequently investigations into the nature of the exchanges of material exhibited by bacteria appears in many cases to afford a safer guide for the determination of definite specific characters than a too rigid adherence to purely morphological features. Such considerations are especially evident in connection with the marked variability of such pathogenic forms as spirillum choleræ, bacillus typhosus or bacterium coli commune. The bacilli of cholera, according to the researches of D. D. Cunningham (1), show no fewer than eight distinct species which differ in form and cultural growth. Even if the contention that these species simply represent nutritive modifications of a single definite form (2) is sustained, it is abundantly clear that great morphological variation may occur. Friedrich (3) has undertaken a detailed study of cholera vibrios and has arrived at the conclusion that the species described by Cunningham are only multiple varieties of Koch's spirillum, which undergoes alterations in form which it is impossible to control; the most abnormal comma-bacilli may at any time take on the character which may be considered typical, that is the form originally described by Koch. A second distinct type of cholera vibrio exhibits longer and more delicate filaments which are frequently twisted into a spiral.

Sanarelli has shown that many varieties or races may connect these two types, and Metchnikoff (4) believes that these types are not constant forms but simply two races which can easily be transformed the one into the other by causing them to pass through leucocytes. Multiple varieties of the pneumococci have been demonstrated by Foà, and other micro-organisms have also been shown to possess great flexibility of form.

According to most bacteriologists the bacterium coli commune, described by Escherich (5) in 1885 as occurring in the excreta of children and animals, is a purely fæcal microbe identical with the form discovered by Emmerich (6) in Naples during the cholera outbreak of 1884, and considered by him to be the specific cause of cholera, a view which at once became untenable when it was shown that this micro-organism is constantly present in both the normal and abnormal contents of the bowel, and could also be isolated from air and putrefying liquids (7). It has been remarked that Escherich's bacillus is so ubiquitous and variable in form and cultural behaviour that at the present time it has usurped the position held years ago by bacterium termo. Still the chief interest of bacterium coli commune lies in its pathogenic character and in a morphological resemblance to the bacillus typhosus, which is so marked that the Lyons School hold that the two bacteria are simply harmless and harmful varieties of a common form. Since typhoid fever is undoubtedly propagated by contaminated water or milk, the importance of establishing definite criteria for proving the presence of specific pathogenic microbes in these liquids is evident, and as soon as it becomes possible to recognise the bacillus typhosus in dejecta with the same certainty as the tubercle bacillus can be demonstrated in sputa a great step will be accomplished in the diagnosis of a disease which in its early stages is not always easy of recognition. Aspiration of the spleen and cultivation of the bacillus typhosus from blood obtained by this procedure has been advocated by Redtenbacher (8) and others as a means of diagnosis in doubtful cases of typhoid fever ; and although this operation is stated to be unattended with bad results,

observations made upon excreta would certainly be more practicable. With the object of establishing a sound differential diagnosis between bacillus typhosus and other microorganisms which are possibly closely allied, a considerable amount of work has been performed, some of which will here be briefly mentioned.

Purely morphological considerations are certainly insufficient to differentiate bacillus typhosus from many other microbes. This bacillus is met with in the tissues in the form of a short, relatively thick rod with rounded ends, which when cultivated frequently grows into threads with a much smaller diameter. Escherich's bacterium and its numerous varieties, the numberless typhoid-like bacilli isolated from water and the bacillus levans recently described by Lehmann (9) may all as far as shape is concerned be mistaken for the typical virulent specific microbe when isolated from the spleen of a typhoid patient. According to Luksch (10) bacillus typhosus exhibits much more lively movements than the bacterium coli, and micro-photographs show that the former of these is provided with 8-12 cilia, which are attached both to the sides and ends of the bacillus, while the latter only possess 1-3. Neither stains with Gram's method and no spore formation takes place. Shortly after the discovery of the bacillus typhosus a sporing stage was described by Gaffky, but this observation has not been confirmed. Recently Almquist (11) considers that spore formation can take place both in Eberth's and Escherich's bacteria. His method for demonstrating this consists in filling Erlenmeyer's or Pasteur's flasks with damp filter-sand or sand soaked with manure. This medium is sterilised and then inoculated with a bouillon culture of the microbe. The flasks are then placed for at least a month in an ice chest. By this time all the vegetative forms have disappeared and minute bodies about $\cdot 5\mu$ are found in the culture medium. He regards these bodies as spores, and describes them, when examined in a hanging drop of bouillon, as growing into rods which again break up into spores. How far these bodies deserve this name is doubtful, and Ilkewicz (12), who by a modification of Kolossow's method has succeeded in staining

the spores of anthrax bacilli so that these appear to each contain a nucleus, finds that under similar treatment the typhoid bacillus and bacterium coli commune only exhibit a simple beaded appearance which has nothing of the nature of spore formation.

Since morphological characters and micro-chemical reactions do not furnish sufficient criteria for the determination of the identity of bacillus typhosus, considerable attention has been given to the behaviour of this bacillus upon various culture media; this microbe is not capable of transference to animals, and therefore this method of diagnosis, which is absolute for the bacteria of such diseases as anthrax or glanders, is excluded. Some observers have published cases of an experimental typhoid fever which follows the introduction of bacillus typhosus into the peritoneal cavity of mice and guinea pigs, but the condition which is established by this method appears to be due to an intoxication with the products of the metabolism of the bacterial culture rather than to a distribution of the microbes in the infected animals that is in any sense comparable with the mode in which the specific bacilli of typhoid fever are found to spread in the human body. In connection with this question the recent papers of Sanarelli (13) and others are of great interest.

Some few years ago the character of bacterial growths on potatoes, in bouillon, gelatine, agar, together with plate cultivations, played an important part in affording differential characteristics for microbes, and it is beyond question that in some cases a determination of a form is possible by these means alone. Since Gaffky (14) first drew attention to the remarkable growth which bacillus typhosus exhibits on potato this has been held to be typical of this specific microbe. A slightly acid reaction of the medium, however, appears necessary, and E. Fraenkel and Simmonds were the first to point out that on many varieties of potato an obvious dirty grey growth occurred, instead of the almost invisible film which is so peculiar. Many observers, and among these Germano and Maurea (15), consider the growth on potato of little value as a diagnostic sign; and the observations of Fuller (16) are interesting, since he isolated no fewer than

five kinds of bacilli from the Merrimac which exhibited a growth on this medium exactly of the same character as that of the typhoid bacillus. Dunbar (17) considers that culture methods generally only complicate the solution of the problem of differential diagnosis of Eberth's bacillus from the bacterium coli commune, and the attempts made in this direction by Uffelmann (37), Vincent (41), Gasser (42), Holz (39), Parietti (40), Chantemesse and Widal, Laruelle (38) and Wurtz (35), which are all culture methods, are, according to this observer, insufficient. Quite recently Inghilleri (43) regards the growth of these bacteria upon amygdalin-bouillon as differential. Bacillus typhosus does not as in the case with the colon bacterium act like emulsin and decompose the glucoside with production of hydrocyanic acid. This effect however is certainly accomplished by other micro-organisms, as Telmi and Montesano have shown; the blood of cholera patients and the dejecta of those suffering from typhoid fever also possess the same property.

Recent papers have again brought into prominence the possibility of establishing a diagnosis between the above bacteria by culture methods. Buchner (18) first pointed out the anti-bacterial action of formalin, though a note on this fluid had been given by Löw (19). This agent has been employed by Schill (20) in order to obtain a differential culture medium. He describes a water bacterium which occurs with Escherich's bacterium in spring and canal water. This resembles bacillus typhosus since it does not coagulate milk, but resembles bacterium coli commune since it develops gas in sterile bouillon and grows on potato with the production of a well-marked, yellowish green crop. To determine the nature of this water bacterium comparative experiments were made to differentiate bacillus typhosus from bacterium coli commune. A formalin-bouillon 1 : 7000 is prepared, and this is inoculated. When working with dejecta it is found that if the bouillon clouds this effect is due to the presence of Escherich's bacterium and is a valuable diagnostic character since bacillus typhosus causes absolutely no turbidity. This fact does not, however, definitely establish the presence of this microbe, since probably other water bacteria

which have the appearance of typhoid bacilli will not grow. At all events the turbidity of formalin-bouillon is decisive against the presence of bacillus typhosus.

Since formalin is only a saturated solution of formyl aldehyde in water, Schill has also examined the anti-bacterial action of this gas. He states that exposure for seventy minutes not only inhibits but kills bacillus typhosus, while bacterium coli commune and typhosus-like water bacteria preserve their vitality after an exposure of over two hours; a longer period inhibits their activity though this is recovered in twelve hours when the action of the vapour is abolished. Under no circumstances does Eberth's bacillus recover after similar treatment. Interesting as these details are, they do not really mark an advance on earlier methods for differentiation such as those employed by Chantemesse and Widal (21), by Parietti and others, where cultivations upon media such as carbol-bouillon, methyl-violet gelatine or fuchsin-agar are largely relied upon as a means of diagnosis.

During the last few years numerous investigators have been occupied in the study of the chemical changes which media show during the growth of these bacteria. Papers by Kiessling (36), Péré (34), Blachstein (32), Germano and Maurea (15) and Dunbar (17) all indicate that the differential diagnosis between Eberth's and Escherich's bacteria must be sought for in the chemical changes which the medium undergoes.

It is allowed that some well-defined differences between the typhoid bacillus isolated from the spleen and the bacterium coli commune taken from the bowel do exist, and though Rodet and Gabriel Roux (22) consider that the latter is capable of being transformed into the specific microbe of typhoid fever this view has found more opponents than defenders. The chief points of interest in differential diagnosis may be referred to here.

That gases are developed when Escherich's bacterium is grown upon weak alkaline bouillon to which two per cent. of dextrose or lactose is added was noticed by Th. Smith (23) in 1889. Independently Chantemesse and Widal (24) made

the same observation, and since that time this behaviour has been held as differential, since Eberth's bacillus causes such a medium to become uniformly turbid in twenty-four hours, but after a few days the medium clears and no development of gases takes place. The fermentation of sugar by bacterium coli commune is stated by Dunbar to yield gas after three hours' incubation at 37°C., and by the end of the fourth day an amount of gas is produced which amounts to about one-third of the volume of the culture medium; this gas consists of carbon dioxide and hydrogen. The new gas-forming bacillus described quite recently by Gärtner (25) from the peritoneal cavity is, according to Klein (26), identical with bacterium coli commune and probably reached this situation by direct migration from the gut. The so-called indol reaction described by Kitasato (27) consists in the addition of 1 ccm. of ·02 solution of potassium nitrite with a few drops of concentrated sulphuric acid to 10 ccm. of a bouillon culture. If this is a pure growth of bacillus typhosus the characteristic red colour seen with cultures of Escherich's bacillus and other typhosus-like bacteria is absent. A reaction described by Zinno (28) and dependent upon the presence of kreatinine is performed by adding a few drops of sodium carbonate and very dilute solution of nitro-prusside of sodium to suspected growths in two per cent. peptone bouillon. An intense red colour develops which gradually fades into yellow. By the addition of acetic acid an emerald green colour appears which passes into blue. These reactions are given by cultures of bacterium coli from many sources but not by bacillus typhosus. Cultures of Koch's spirillum and vibrio Metchnikovi respond to the above tests, which is not the case with the spirillum of Deneke or that of Finkler-Prior.

The behaviour of the typhoid bacillus and allied organisms when cultivated upon sterile milk was first noted by Chantemesse and Widal (29), who established the fact, which has been verified by all subsequent observers, that the bacillus typhosus does not cause coagulation of milk, while this is a constant feature in the growth of bacterium coli commune. In connection with this Eberth's bacillus, though it

will grow upon an acid soil, produces very little lactic acid when cultivated upon sterile whey, while, as Petruschky (30) originally discovered, the bacterium of Escherich produces more than twice the amount of acid. Milk coagulation is apparently due to this development of acid and not to an enzyme, and Huysse (31) has shown that cholera vibrios also possess this property of acid production with consequent curdling of milk.

The researches of Blachstein (32) which were carried out in Nencki's laboratory have added largely to our knowledge of the biology of the bacillus typhosus. The lactic acid produced by the growth of Eberth's bacillus on glucose bouillon is always lævo-rotatory and no ethyl alcohol is produced. The bacterium coli commune grown upon a similar soil produces ethyl alcohol and considerable quantities of dextro-rotatory paralactic acid. Macfadyen (33), while working with Nencki, examined the contents of the small intestine chemically and also for bacteria. These he found especially attacked the carbohydrates within the bowel. Alcohol was formed by all the isolated microorganisms and the amount produced in some cases amounted to sixteen per cent. of the weight of the sugar. The lactic acids present in the bowel contents were either the especially inactive or the dextro-rotatory paralactic acid. There was apparently no evidence that any other lactic acid was present. Blachstein speaks of three different activities which the bacillus typhosus can exhibit according as this microbe is isolated from dejecta, from the spleen, or taken from subcultures. The first and last of these kinds produce relatively much and little lactic acid, but this is always lævo-rotatory. Calculations have shown that the small intestine of the guinea pig contains about 1400 microbes, and the large intestine 2000-5000 for each decigramme of material. Among this vast number it may be affirmed that no single microbe in the normal gut yields a lævo-rotatory lactic acid, and since Escherich's bacterium is a normal occupant of the gut of infants, men and many animals, it may be confidently affirmed that typhoid fever cannot be spread by normal human dejecta, a view which in this

country, owing to the teaching of Murchison, is still frequently entertained. A transformation of bacterium coli commune into a typical bacillus typhosus is, according to Blachstein, impossible. These allied micro-organisms, which are probably specifically distinct, though the determination of this can only be decided by methods other than morphological, are likely to arouse even greater interest in the future, since it has been shown by Chantemesse and Widal that Escherich's bacterium, though not directly pathogenic for man as it is for some animals, aids greatly in the growth and generalisation of bacillus typhosus in an infected body.

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FOSSIL ALGÆ.

THERE cannot but be general agreement in the view that all theories of the evolution of plant forms must be based on the assumption that Algæ are among the least changed descendants of the earliest forms of life on the globe. They represent an extreme term in the series of autonomous organisms capable of converting inorganic matter into food substance, and though organisms of more simple organisation are known, these are parasitic or saprophytic and accordingly may be, must be, accounted for on the same theory, as derived by degeneration from forms such as Algæ vegetating by their own intrinsic chlorophyll, using the word in its widest sense. Since successive researches of absorbing interest made during recent years have declared an express complexity of cell-organisation even in the simplest forms of Algæ we may further proceed to assume without violence the possibility of the existence even now, or, at all events, in past ages, of still more elementary organisms of like autonomous character representing a more remote ancestral type. However far back we may push such speculative conceptions of elementary organisms it is plain that whether they did or did not exist in past ages, their bodies must have been so little specialised in the direction of stable structures that we cannot hope for their preservation in fossil forms. The student of the primordial forms of life must therefore content himself with fossil Algæ as the representatives of the earliest type, and he is driven to this by another consideration. It is of course conceivable that more primitive organisations than these should occur in fossil form, but from the nature of things we should not be justified in assigning them to the main series of independently vegetating organisms, since, as has been said, we already know simpler forms of degenerate type with which they might be classed with equal propriety so far as the remains of cell-walls, etc., could guide us.

An examination of the testimony of the rocks to the existence in past ages of Algal forms is therefore a process of general interest to biologists and to all who are concerned with the subject of evolution. The most superficial consideration of the matter will result in the recognition of the high degree of improbability that attends the chances of preservation of such delicate structures as the cell-walls of Algæ. Acquainted as we are with the marvels of preservation of minute structure in many plant fossils, exhibiting even cambial cell-walls as in a newly cut section of a living plant, there must yet be borne in mind the rarity of such conditions in geological history, and their limitation, with a few exceptions, to the case of terrestrial vegetation, though aquatic agencies may have operated in its preservation. Prepare ourselves as we may by such antecedent considerations, a revelation of the scantiness of the record comes with an uncomfortable shock, and the object of this article will be attained if the shock act as a stimulus to the undertaking of new searches for material in the older rocks, since the bareness of the record is doubtless due in part to the want of enterprise of this kind. There is a disposition to be content with the reflection that the older the organism the simpler it must be and therefore the less likely to be preserved to us, that the very absence of such forms from fossiliferous strata is an eloquent comment on their character, that there is no use fighting against the malignant conspiracy of natural forces to obliterate the vestiges of early life, but such a disposition is a truant one and inconsistent with the progress of palæontology in general.

By the researches of Nathorst (1 and 2) the ground has been cleared of the debris of many spurious fossil Algæ described by Brongniart and other older writers under the names of *Chordophyceæ*, *Arthrophyceæ*, *Rhizophyceæ*, *Spongiophyceæ*, *Dictyophyceæ*, *Keckia*, *Münsteria*, *Oldhamia*, *Eophyton*, *Discophorites*, *Gyrophyllites*, *Chondrites*, *Confervites*, *Caulerpites*, etc. A considerable number of such forms were thus confidently described and assigned definite places among the Algæ by writers who must have had vague enough notions of the outward forms of the living organisms.

Nathorst pointed out after obtaining similar markings by experiment that many of them were mere trails of animals and other casual impressions, or the remains of other organisms, and while thus sweeping away an encumbrance of little credit to botanical literature he laid down the useful rule that the claims of no organism of the kind should be accepted unless it exhibit actual structure, or, at all events, a rind of coal. That this excellent rule is somewhat too severely exclusive has been pointed out by Graf zu Solms-Laubach (7), since coal "may entirely disappear in the course of time from remains that are undoubtedly organic, if they are deposited in a porous rock". A literal application of it would affect many very definite and characteristic impressions of fossil plants, but there is no gainsaying the healthy nature of his conditions. Any departure from them must be supported by strong evidence and properly safeguarded by a consideration of the nature of the bed as disclosed by other remains. Unfortunately the evidence of impressions of outward forms is little to be trusted in the case of Algæ, since few groups of them exhibit steadfast and characteristic outlines owing to their extreme plasticity in response to their environment. Nathorst's useful results were strongly disputed by the Marquis de Saporta (3), who furnished his own condemnation in the illustrations to his memoir. These examples of astonishing innocence and credulity as to the characteristic forms of Algæ are an eloquent plea for the rigorous employment of Nathorst's conditions. (The reader is referred to Solms-Laubach's *Fossil Botany* (7) and to Zittel's *Handbuch der Palæontologie, II. Abth. Palæophytologie* (8), commenced by Schimper and completed by Schenk, for further details and references to the literature of this portion of the subject.) We are left, then, with a small residuum of organisms, which possess, however, the exceptional interest of being our only known remains of the vegetation of past oceans, their shore Algæ and their plant Plankton.

The great number of Algæ existing at the present day, their variety of form and range of habitat, would lead one to

expect a considerable series of representatives in the fossil-bearing strata. On the other hand, their simple structure and their extremely rapid decomposition form weighty reasons for expecting little, and it will be seen that those Algæ now exhibiting special adaptations of structure to durability are precisely the groups of which representatives are preserved to us. The Diatoms with their siliceous walls, the calcareous *Siphoneæ* and *Corallineæ*, are the Algæ about which one would now be most inclined to prophesy on grounds of structure that their remains are most likely to be embalmed in the deposits now forming in the ocean, and they are all of them in fact found in sediments of contemporary age. If we add the encrusted *Characeæ* as sharing in the chances of preservation no violence will be done to reasonable expectation. It is a very remarkable fact that after the Augean labour of Nathorst and others the only fossil Algæ of any importance left to us belong to these very groups. They are almost the only Algæ of our present seas of which the structure is rendered fairly permanent by mineral encrustation during life. May we not legitimately suppose that in past ages their less resistant companions, organised as they are at the present day, suffered such extinction as it may be presumed they now undergo? If, however, we look more closely into the record our expectations will be enhanced of finding other forms.

The first fossil Alga exhibiting structure and so furnishing adequate claims to recognition appears in the Devonian age, *viz.*, *Nematophycus* of Carruthers, and (with the exception of the doubtful *Pachytheca* of the same age) it stands alone. It was first described under the name of *Prototaxites* by Sir William Dawson, who took it for the wood of a Gymnosperm, but the subsequent examination of it by Mr. Carruthers (4 and 5) dispelled this interpretation and established its claims as an Alga. He placed it among the *Siphonaceæ* (especially *Udoteæ*) and beyond doubt correctly. It is hard to see on what ground Solms-Laubach suggests *Fucaceæ*, since the great uninterrupted lumina of the tubes and the fine lateral haptera or tenacula are wholly inconsistent with the structure of any Fucaceous

plant. It must have formed a noble sea-weed with stems several feet in circumference recalling in stature the Fucaceous and Laminarian giants of our southern seas, and in girth at all events surpassing them. There is no evidence of calcification here, and the presence of tenacula would secure the cohesion of the tubes which are not so much interwoven as those of *Avrainvillea*, for example, which dispenses with both calcification and tenacula. Since *Udotea*, however, which it most resembles, possesses encrustation nearly always slight but varying in amount in different species, in addition to tenacula, it is possible that we may owe the preservation of *Nematophycus* to a slight calcification though no trace of it remain. Disregarding *Pachytheca* as a doubtful case we must leave the primary rocks with this sole veritable representative of the Algæ.

It is difficult to say definitely what is to be made of Bertrand and Renault's *Pila bibractensis* (6) of the Permian epoch. Its authors describe it as a gelatinous Alga with an ellipsoid, multicellular thallus, and appear to see farther into its nature than one is quite prepared to accompany them. It is impossible to assign it a place, and there is in point of fact little to be said and more to be doubted with regard to this remarkable production.

In the great sweeping away of spurious fossil Algæ the Secondary rocks were left quite destitute of any true claimant to recognition until we come to the top of the series in the Cretaceous beds which contain diatoms and *Lithothamnion* (Senonian beds). The forms of *Bactryllium* of Triassic age may eventually be found to survive this denial of their claims as Algæ, and the possibly Siphonaceous *Diplopora* from the Muschelkalk and Lower Keuper, *Munieria* and *Triploporella* from Cretaceous beds, are even more likely to receive adoption into the series of true Algæ. The present writer described (9) a fossil *Caulerpa* (*Siphoneæ*) from the Oolite (Kimmeridge Clay) of which very complete casts in the round are preserved, and it is noteworthy as the remains of an Alga which was not encrusted with any mineral secretion. In these last days Mr. Seward (10), in his admirable account of the Wealden flora given in

his *Catalogue of the Mesozoic Plants* in the British Museum collection, has made a further contribution to the subject. He proposes the generic term *Algites* "for those fossils which in all probability belong to the class Algæ, but which by reason of the absence of reproductive organs, internal structure, or characters of a trustworthy nature in the determination of affinity, cannot be referred with any degree of certainty to a particular recent genus or family". At first sight this appears to be a retrograde movement, but on the whole it is a wise conservatism, and the only danger I can foresee is in the genus being made a convenient limbo for fossils which by no probability belong to the Algæ—new *Eophytons* and *Spirophytons* and the like. Mr. Seward will have to jealously guard a genus of such elastic characters. No one can withhold sympathy from the cautious step he has taken, since it will no doubt often happen in the future that fossil remains indicating Algal nature will come to be recorded by men who would shrink from calling them by generic names that suggest modern affinities like *Caulerpites*, *Chondrites*, etc. It is very much the same position as that adopted early in this century by Dawson, Turner, Robert Brown and others in the study of the living forms. They retained the name *Fucus* for hosts of Algæ which they knew to be far other than congeneric until a proper system of classification could be established. Mr. Seward describes *Algites Valdensis*, an Alga with the dichotomous habit of *Chondrus crispus*, *Dictyota*, *Nitophyllum*, etc., among living forms and *Algites catenelloides*, the specific name of which describes its resemblance to a well-known Floridean form. Mr. Seward also describes the oogonia of a *Chara*, *viz.*, *Ch. Knowltoni Sew.*, and he reminds us of another *Ch. Jaccardi Heer* recorded from the Wealden and another species of Saporta's from the Jurassic system. It is highly probable, however, that these forms ought properly to join the assemblage of problematic Charas that extend back to the Lower Devonian Carboniferous sandstones. Another *Chara* of Secondary age has recently been described by Knowlton (as noted by Mr. Seward) from the Upper Cretaceous Bear River formation of North America. Mr. Seward's own

Chara strikes me as much the best claimant until we come to the Tertiary *Characeæ*.

In passing one ought to note the possible Alga of the Oolitic granules at present attracting the attention of many geologists and resembling the *Girvanella* of Nicholson and Etheridge of Ordovician age, and as Mr. Bullen Newton points out to me the *Siphonema* of Bornemann which goes as far back as the Cambrian rocks. The best specimens reveal no more than a tubular structure consistent with an organic origin, and however strong the temptation may be to regard them as Algal such recognition must be delayed until better evidence is forthcoming.

Castracane has stated that he found several species of diatoms in the ash of English coal and that these are fresh-water forms at present existing. So exhaustive a search has been made, however, by Williamson and other observers that in spite of the fact that Castracane claims to have used due precautions we must treat his record as open to doubt until it is confirmed by further discovery. No diatom appears with this possible exception until we reach the Upper Cretaceous beds, and then they occur again in extensive deposits of Tertiary and Quaternary age. All these fossil diatoms, and they are very numerous, from the chalk downwards, belong to genera and a large number of them to species now existing, though the proportion of identical species diminishes with the age of the deposit. The forms known as *Bactryllium* from the Trias may have been ancestors of the diatoms—it is very doubtful—and but for this one possibility we have no hint of the coming of this type until we find it in a profusion of forms some of which have survived from the Cretaceous age to the present day. One would expect them in the Silurian rocks, but here too there is a blank. Careful search has been made for them by several observers, but the matter is eminently worthy of prosecution and may be urged upon the characteristically industrious diatomist as a more worthy occupation than practical experiments in the origin of species.

Just as at present the diatoms are engaged in making deep-sea deposits in the colder regions of the northern and

southern oceans, the Rhabdospheres and Coccospheres, which we must regard as pelagic Algæ, are playing the same *rôle* in the tropical and temperate seas. The broken-down parts of these organisms known as Rhabdoliths and Coccoliths are found in the globigerina oozes, and they have practically the same geological history as the diatoms. The problem presented by these remarkable organisms is undoubtedly of palæontological interest, but it is primarily a biological one since so little is known of the minute structure and mode of life of the contemporary forms.

Though *Lithothamnion* appears first in the Senonian (Cretaceous) beds it may eventually prove to be as old as the Muschelkalk (Trias). It is not, however, until we reach the Tertiary rocks that this coralline is found occurring massively, as it does in the Lower Eocene. The Leitha limestone (Miocene) and Pisolite limestone and the Nummulitic rocks owe their origin in great part to this contemporary genus. Every botanist who has waded over a coral reef must have been struck by the massive occurrence of this rock-building Alga, the activity of which has been somewhat neglected by students of coral reefs. I think it was Mr. Darwin who remarked that it often formed the cement that bound the coral together. It frequently does more than this. A large number of the specimens brought back by Mr. Bassett Smith, R.N., from his survey of the Macclesfield Bank were *Lithothamnia*, and to witness what this Alga can do in forming a beach in the absence of coral one need not go beyond the British Islands. It is the only Floridean fossil of certain determination unless Mr. Seward's *Algites* should prove to be of this group.

The fossil *Dasycladaceæ* which we owe to the researches of Munier-Chalmas (11 and 12) are of an importance far exceeding all other results in fossil Phycology. With a power of extraordinary divination this author has rescued from among the fossil Foraminifera and elsewhere a series of Tertiary and other Algæ, and having accomplished this remarkable scientific feat has maintained an almost equally remarkable reticence on the subject. In fact we owe to

Graf zu Solms-Laubach and others the better part of the interpretation of the matter. The best defined forms, such as *Polytrypa* from the Grobkalk (Eocene) = the existing *Cymopolia; Uteria* of the same type from the Lower Eocene; *Haploporella* of the type of *Neomeris* and *Zittelina* and *Terquemella* answering to our *Bornetella* of the present day; the more puzzling *Thyrsoporella* and *Prattia, Marginoporella, Dactylopora,* etc., are all Tertiary and chiefly Eocene but occur also in Oligocene and Miocene deposits. Of the same age is *Acicularia*, a genus closely allied to our modern *Acetabularia*, and *Ovulites*, which Munier-Chalmas thinks near *Penicillus* (a Siphonaceous genus outside *Dasycladeæ*) but Solms-Laubach justly determines to be close to *Cympolia*. One may anticipate most hopefully the certain extension of this series backward through Cretaceous, Jurassic and Triassic rocks, and recognition of such forms as *Munieria, Triploporella, Gyroporella* and *Diplopora annulata* (see Gümbel, 13) referred to above, as members of the same series. These forms from the Secondary rocks, however, are fewer and much less definite, but a most fertile field has been opened up here for further investigation, and its fruits are for the cautious investigator, who above all knows his living Algæ.

In the Tertiary rocks we have then representatives of the corallines, *Siphonaceæ, Characeæ* and many diatom deposits, and to these may be added the possible *Cystoseiræ* (*Fucaceæ*) of the Radoboj beds (Oligocene).

The Quaternary rocks continue the history of fossil diatoms and *Characeæ*, and our record closes with Borge's discovery (14) in the glacial clays of the Island of Gotland of a number of Desmids identical with types now alive in the Arctic regions.

The two papers by Mr. James (15 and 16) are mainly of bibliographical interest. He has made a study of the wrecked genus *Fucoides*, its origination by Brongniart, and the numerous additions made to it at various times. The palæontologist will be glad of this service, since the "problematic organisms" in question are often as obscure in

their nomenclature as they are dubious in botanical character.

The most hopeful direction for work then appears to be in the search for further encrusted Algæ, for more corallines, *Udoteæ*, for diatoms in the older rocks and perhaps for other *Florideæ* such as *Squamariaceæ* of which no record has yet been obtained, though the thallus has an equal chance with some of those mentioned. The Cretaceous seas had a plant Plankton like our own in the matter of diatoms and Coccospheres and Rhabdospheres and the older Secondary rocks may yet prove productive of more and better defined forms. The example of Munier-Chalmas is inspiriting, and by following it the great periods laid waste by the successful destructive criticism of Nathorst and others may yet be re-peopled with forms having valid claims to recognition as true fossil Algæ.

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ANCIENT VOLCANIC ROCKS.

THE continental petrologists, with few exceptions, continue to make a fundamental distinction between the "older" (*i.e.*, pre-Tertiary) volcanic rocks and the "younger" (Tertiary and Recent). The hold which this doctrine has maintained in almost every country, but our own, depends upon a conjunction of causes. One of these causes is the almost complete absence in the European area of volcanic activity during Mesozoic times. Owing to this long hiatus, the fresh Tertiary lavas come to be, as a whole, strongly contrasted with their much older Palæozoic equivalents, which in course of time have usually suffered much from the secondary alterations to which volcanic products are peculiarly liable. By regarding the accidental characters thus induced as essential, an impression of wide difference between the older and the newer lavas is fostered.

Another curious fact has contributed to confirm the same idea. Germany, France, and Italy form part of a region within which the Tertiary volcanic rocks belong, in great measure, to somewhat peculiar types. Their relative richness in alkalies frequently gives rise to special minerals and associations of minerals; and the occurrence in other parts of the world of corresponding rocks having a high geological antiquity has been the more easily overlooked since some of these special minerals, such as leucite, are eminently liable to chemical destruction.

The doctrine of "older" and "younger" volcanic rocks, itself a surviving relic of the theories of Werner, has been fortified by these circumstances, and is incorporated in the petrological classifications and nomenclature current in continental countries. The leaders in this branch of geological science have, it is true, seen reason in some instances to modify their views; but it is a singular illustration of the conservative spirit that, even while formally abandoning the idea that the essential characters of igneous rocks depend on their geological age, they still retain it in

their text-books and in their terminology, and consider "Carboniferous trachytes" and "Ordovician rhyolites" unpardonable solecisms.

In this country, although it has contributed far less than Germany to the enlargement of the practical knowledge of igneous rocks, the early and full acceptance of Huttonian doctrines has permitted of somewhat wider views on some questions. Moreover the attention of petrologists has been especially directed to the great groups of Palæozoic volcanic rocks so fully represented in the British Islands, and so, necessarily, to the comparison of them with the Tertiary lavas which fill so large a space in petrological literature. The result has been a revolt against the above-mentioned dogma and a well-founded conviction among English students that the supposed differences between the older and the younger volcanic rocks reduce to the fact that the former are, as a rule, more affected than the latter by the changes which come with lapse of time.

Some part of the difficulty seems to have arisen from confusing volcanic with plutonic rocks. The latter being formed under deep-seated conditions and brought to light only by long-continued erosion, those actually seen belong for the most part to pre-Tertiary times; and indeed the geologists who still cling in some degree to Wernerian ideas have only reluctantly come to admit the existence of granites, gabbros, etc., of Tertiary age. Among the older strata it is not always easy to distinguish, by field-evidence alone, between intruded and contemporaneous igneous rocks. Hence, when it is stated by some writers that certain minerals, such as muscovite, microcline, rutile, tourmaline, and topaz, are found in the older but not in the younger rocks, a more correct form of statement would be that these minerals are characteristic of plutonic rather than volcanic rock-types. In any form the statement is not true without qualification. Hypersthene is another mineral which was once considered to be characteristic of the older rocks, it being known at that time as a rock-constituent only in the large and usually "schillerised" crystals in which it occurs in hypersthenites and gabbros. Whitman Cross in 1883

showed that hypersthene in small crystals of very different appearance is a constituent of widespread occurrence in Tertiary andesites, and it has since been recognised as equally abundant in andesites of Palæozoic age.

The minerals which have been considered peculiar to the Tertiary volcanic rocks are leucite, nosean, haüyne, melilite, and tridymite, and some of these are still scarcely, if at all, known in the older lavas. Considering, however, the rarity of melilite-basalts and haüyne-bearing rocks among the Tertiary and Recent lavas, it is not a matter for surprise that they should remain unknown among lavas which have received less attention.

What is perhaps more significant is the fact that all these minerals are peculiarly liable to decomposition, and of some of them, at least, all trace would easily be obliterated by secondary changes in the rock. The case of leucite is especially interesting, for here, although the mineral is commonly destroyed in the older rocks, its unmistakable crystal-form is in some instances clearly preserved by its decomposition products. Of this nature seem to be the "pseudo-crystals" composed of orthoclase and nepheline described by Hussak and Derby in the phonolite and foyaite of Tingua Mountain in Brazil. Opinion was somewhat divided as to the nature of these pseudo-crystals, but doubts may be considered to be set at rest by the study of similar pseudomorphs in other districts, and especially by J. F. Williams' description of the leucite-syenite of Magnet Cove in Arkansas. If the large leucite-crystals in such rocks have become obscured, it is easy to understand how the small crystals of volcanic rocks may have been altogether obliterated.

Palæozoic leucite-lavas, however, are not wholly unknown, and one has been described by Von Chrustchoff (1) from Siberia. It occurs on the right bank of the Tunguska, forming a flow distinctly overlain by limestones containing *Favosites*, *Halysites*, *Cyathophyllum*, and other characteristic Lower Palæozoic fossils. Little white crystals of leucite, up to 1 mm. diameter, are visible in a compact groundmass, and the microscope shows augite, anorthoclase, sani-

dine, nepheline, and accessory constituents, with some residual glassy base.

As regards nepheline, this mineral, like some others, is known in small perfect crystals and in larger and more shapeless ones with numerous inclusions (elæolite), and these two varieties are now recognised as characterising, not "younger" and "older" rocks, but volcanic and plutonic rocks, respectively.

Again, it has been alleged that the sanidine variety of orthoclase and the microtine varieties of the plagioclase felspars are restricted to Tertiary and Recent volcanic rocks. Sanidine is not very strictly defined, and the name is often used to imply merely a fresh glassy appearance, the loss of which in the felspars of the older lavas is easily explained by incipient chemical decomposition. In some instances, however, undoubted Palæozoic rocks contain felspar showing the characteristic crystal-habit, the orthopinacoidal cleavage, and the glassy lustre of typical sanidine, and the distinction is clearly one upon which no stress can be laid.

It is, of course, well known that many of the Tertiary lavas contain a considerable amount of isotropic glassy base, and some of them (obsidians) consist almost wholly of glass. The supposed absence of these glassy types among the older formations was pointed out by those who maintain an essential distinction between the pre-Tertiary and the Tertiary lavas. The answer was given by Allport, to whom in the first place belongs the honour of clearly seeing and upholding the essential identity of corresponding volcanic rocks of all ages. In the group of volcanic rocks near Wellington in Shropshire, now known to be of pre-Cambrian age, he pointed out (1877) all the characteristic structures of the fresh Tertiary rhyolites of Hungary, etc., still evident in rocks which have lost their glassy nature by molecular changes. These ancient obsidians and rhyolites show, in different examples, trichites, perlitic cracks, spherulites, etc., and they are accompanied by rocks which are recognised as altered volcanic ashes. Rutley and others have shown that similar *devitrified* acid lavas have a very wide distribution in the Ordovician of North Wales,

Westmorland and other districts, and the work of Bonney and Cole makes it appear that even such special structures as the curious lithophysal cavities in some fresh obsidians find their analogues among these ancient rocks.

Allport had already shown (1870, 1874) that the basic rocks—basalts and dolerites—of the British Isles, of late Palæozoic age, are, except for secondary changes, mineralogically and structurally identical with the late Tertiary rocks of similar composition in Europe. Other English authors have described our older intermediate lavas with the same general result, Teall's study of the Cheviot hypersthene-andesites, of Old Red Sandstone age, being especially noteworthy (1883). Most of these rocks are considerably altered, and have been known in the field under the name "porphyrite," a term largely applied to altered andesites in other districts; but one rock, the "pitchstone-porphyrite" of some authors, is relatively fresh, with partially glassy base, and closely resembles the Tertiary andesites of Hungary and the lavas erupted from Santorin during the present century. A closely allied type has since been recognised in the Ordovician of Carnarvonshire.

Some of the most interesting of recent contributions to our knowledge of ancient volcanic rocks come from the Carboniferous districts of Scotland and Ireland. In the earlier part of the Carboniferous period the area which is now Southern Scotland experienced a great outpouring of volcanic material. The discharge of lavas was so copious as to give rise now to broad table-lands and ranges of hills, sometimes many hundreds of square miles in extent, and Sir A. Geikie (2) distinguishes this phase of vulcanicity as that of the "plateau" eruptions, contrasting these with the more restricted and sporadic "puy" eruptions of a somewhat later time in the same area. He enumerates five plateaux, marking as many independent centres of volcanic activity, the original extent of the largest being estimated at between 2000 and 3000 square miles. The materials representing these great eruptions are mainly successions of lava-flows, but with subordinate beds of tuff. The greater part of the lavas have not yet been closely studied, and are designated

by the general field-term "porphyrite". Judging from the variety of rock-types recognised by the examination of the small plateau of the Garlton Hills in East Lothian, it seems probable, however, that the whole group will be found to yield many results of interest.

Some of these Garlton Hills rocks have been described by Hatch (3). They are partly from lava-flows, partly from "necks" which are believed to mark the actual sites of volcanic vents. In some cases the rocks have retained their original characters with remarkable freshness. The lower lavas are of thoroughly basic composition, and, for the most part, are olivine-basalts with from 46 to 49½ per cent. of silica. One, however, is of ultrabasic nature, with only 40 per cent. of silica and large proportions of lime, magnesia, and ferrous oxide. Microscopical examination shows it to be a well-marked type of limburgite or magma-basalt, comparable with those of the Kaisertuhl and other districts. Felspar is unrepresented, except by an occasional skeleton-crystal. The olivine is often quite fresh. The augite is a titaniferous variety, showing the characteristic violet tint and pleochroism. The crystals of these minerals, with some magnetite, lie in a ground-mass consisting of augite-microlites and glassy matter, and some nepheline is probably present.

The upper lavas are equally interesting, being chiefly unaltered trachytes with 58½ to 62½ per cent. of silica and 10 per cent. of alkalies. They are holocrystalline rocks, sometimes markedly porphyritic, and consist essentially of fresh felspars with a small proportion of a green soda-bearing augite or ægirine. They are indistinguishable from many Tertiary ægirine-trachytes. Some of the volcanic necks in the district, such as North Berwick Law and the Bass Rock, are formed of trachytes not materially different from those of the flows; but one of them, Traprain Law, shows an interesting difference. It consists mainly of little sanidine-prisms with crystals of a bright green soda-augite, but there are also little patches which close examination proves to consist of nepheline and its alteration-products. The rock is therefore a phonolite, though of a type

poor in nepheline and approaching the trachytes in character.

It is to be expected that further examination will discover lavas of these various types in other parts of Britain. Watts has already noted in the Limerick district a limburgite lava, also of Carboniferous age, and Hobson (4) has described from a neighbouring locality the allied type augitite, in which olivine as well as felspar is wanting, the rock consisting essentially of two generations of augite and magnetite with some base which has probably been glassy.

Meanwhile individual geologists in Germany and other countries have recognised the importance of secondary changes, such as devitrification, in discussing the characters of the older volcanic rocks, and much light may be expected to be thrown upon the various "porphyries," "ceratophyres," "porphyrites," "melaphyres," "schaalsteins," etc., when they come to be compared, from this point of view, with recent lavas. Some of the researches of this kind already published have a special interest as referring to what may be regarded as classical districts for geologists.

In this connection we may profitably notice Sauer's (5) work on the pitchstones and porphyrites of the Meissen district in Saxony. These form respectively the lower and upper parts of a series of lavas, both underlain and overlain by tuffs, and Sauer's survey conclusively establishes the true volcanic nature of the whole. The data are wanting to fix their precise age, but they almost certainly belong to a late epoch in the Palæozoic. The well-known "pitchstones" are acid rocks containing only scattered crystals in a ground essentially of glass. Minute granules of black iron-ore occur in the glass, sometimes aggregated in rows. Large spherulites are met with in one type. Perlitic cracks are universally present. A feature which several observers have noted is the frequent occurrence in the glassy ground of irregular, cloudy or "microfelsitic" patches. Sauer points out the relation of these patches to the perlitic cracks, a clear evidence of their secondary origin, and regards them as due to the devitrification of original glass. By the spreading of this change, the whole of the glassy ground

becomes replaced by the "microfelsitic" substance, the original perlitic structure being sometimes still traceable, like the veins in the mesh-structure of serpentine derived from olivine. More interesting is the tracing of a passage from this type into the "Dobritz porphyry," which had formerly been regarded as a quite distinct rock. In the microcrystalline ground of the latter rock occasional relics of the perlitic structure are still preserved, as well as the microlites of iron-ore, now converted to limonite, and in places spherulites. The evidence of conversion of a glassy to a cryptocrystalline and finally to an evidently microcrystalline rock seems to be complete at every point.

An interesting question raised is that of the relation of devitrification to dehydration. The author considers that in the first stage of devitrification of the pitchstone there has been, not a loss, but a certain gain of water. In the passage to the microcrystalline state, however, the rock has lost most of its water, and the consequent contraction has given rise to crevices and hollows.

The so-called porphyrites of the district vary from what is described as a normal mica-porphyrite to one comparatively rich in quartz. They are almost all greatly decomposed; but one, termed porphyrite-pitchstone, is relatively fresh, with an unaltered glassy base. It contains enstatite, as well as plagioclase, quartz, hornblende, and biotite, and if it occurred in Hungary would doubtless be named enstatite-dacite. The author gives reasons for believing that this rock represents the original type of the whole group, the microcrystalline ground-mass of the majority being a result of devitrification. His researches therefore go to establish that not only may quartz-porphyries (in the descriptive sense) be derived from glassy rhyolites, but also quartz-porphyrites from glassy dacites or quartz-andesites.

The most complete account yet given of a limited group of ancient volcanic rocks is perhaps Mügge's (6) description of the so-called "Lenneporphyre". These rocks, occurring in the Middle Devonian of the Lenne district in Westphalia, have been the subject of much discussion. Mügge shows conclusively that they are contemporaneous igneous rocks,

in part lavas, in part fragmental accumulations. The lavas are distinctly acid rocks with a general preponderance of soda over potash, and may thus be described as rhyolites or soda-rhyolites. Following the German terminology, the author styles them *Keratophyre*. They are divided into quartz-ceratophyres and felsite-ceratophyres, according to the presence or absence of porphyritic quartz, although the latter rocks are scarcely less acid than the former. One type has abundant porphyritic crystals of corroded quartz and alkali-felspars with flakes of pale mica, which is not muscovite but a bleached biotite. There are sometimes large nodular bodies (giant spherulites) with a central hollow, and there may be relics of a spherulitic structure on a smaller scale, but the ground-mass is believed to have been largely glassy. It has now completely lost its original characters, and in some cases consists mainly of quartz granules. There has evidently been not mere devitrification, but an introduction of silica, which is proved by the high silica-percentage of the rock, reaching as much as $82\frac{1}{2}$. There are little veins of opal in the ground, as well as quartz. From this massive rock every gradation is seen to a schistose and very sericitic kind of the so-called "porphyroid," which here, as elsewhere, is proved to result from crushing.

Another type is much poorer in porphyritic crystals. The ground-mass, though containing veins and nests of secondary quartz, is richer in orthoclase than the preceding, and usually shows little altered spherulites.

The felsite-ceratophyres lack the quartz and mica of the former types. Soda preponderates greatly over potash in the analyses, and the silica-percentage is about 79. All these rocks show a strongly marked flow-structure, which passes uninterruptedly through certain large skeleton-spherulites. The latter (closely paralleled in some of the Welsh Ordovician rhyolites) sometimes make up almost the whole bulk of the rock. These rocks too are subject to crushing, and, as in the former types, the schistose varieties have not the high silica-percentage of the uncrushed.

Mügge makes a minute study of the fragmental rocks associated with the lavas. Some of them are pure tuffs consisting largely of ash-particles, which often show the characteristic concave outlines suggestive of comminuted pumice. In others, which the author styles "tuffites," the ashy particles are mixed with ordinary sedimentary material. The paper, illustrated by seven plates of photographic figures, is a valuable addition to our knowledge of the older lavas.

The Swedish geologists have in recent years described various ancient lavas in every way comparable with the rhyolites, etc., of Tertiary volcanic districts. Such, according to O. Nordenskjöld (7), are the "hälleflintas" which form part of the Archæan formations in the south-east of Sweden. He separates them from the ore-bearing sedimentary "hälleflintas" of Central Sweden, and shows that many of them have "characters in which they agree with the ancient English felsitic rocks, which have been described by the authors of that country as, in part, rhyolites and devitrified obsidians". The correspondence is indeed very close, and is only occasionally obscured by a certain schistosity due to crushing. The rocks are usually porphyritic with crystals of plagioclase, orthoclase, and less frequently quartz. The ground-mass varies from cryptocrystalline to somewhat coarsely crystalline, and frequently shows fluxion and banding, eutaxitic structure, microspherulitic portions, and other characteristic features of the acid lavas. In some of the rocks a cryptocrystalline ground-mass shows regular perlitic cracks occupied by secondary minerals, besides trichites, margarites, and grouped crystallites: these are obviously devitrified obsidians. Very interesting are the so-called "conglomeratic hälleflintas". The supposed pebbles are found to be really altered large spherulites with concentric shell-structure and central space occupied by calcite, quartz, etc., and the description corresponds identically with those of the "nodular felsites" or altered coarsely spherulitic rhyolites so well known in North Wales.

In Central and Littoral Sweden and across the Gulf of Bothnia, in Finland, Högbom (8) has recorded volcanic

rocks belonging to several distinct types. They are post-Archæan but apparently pre-Cambrian, and seem to be in part superficial lavas, in part volcanic dykes. They include syenite-porphyry (a porphyritic trachyte), augite-porphyrites (augite- and hypersthene-andesites), melaphyre (olivine-basalt), and spilite (amygdaloidal andesite). Judging from the descriptions and photographic figures given, these rocks show only the ordinary changes, such as the conversion of olivine to serpentine and hypersthene to bastite and the filling of the vesicles by secondary products, all essential characters being beautifully preserved.

We may mention in passing the remarkable occurrence of acid volcanic rocks, in a perfectly fresh condition, on the shores and islands of Lake Mien in the south of Sweden, where they occur surrounded by gneisses, granite, diorite, etc. These rocks were discovered as blocks and *in situ* by Holst (9), and have been microscopically described by Szádeczky (10). They include glassy and microspherulitic rhyolites, rhyolitic tuffs and breccias, etc., and have not suffered devitrification. In this case, however, there seems to be no clue to the age of the rocks, and the eruption is probably to be referred to no very distant epoch.

Our knowledge of the older volcanic rocks is perhaps less complete as regards the basic than as regards the acid types, but Barrois (11) has given a valuable description of a series of basic eruptions of Lower Palæozoic age in Brittany. The rocks, which we should term dolerites and augite-andesites, with associated tuffs, are developed in the Menez-Hom district in the department of Finistère, and belong to several horizons in the Ordovician and Silurian systems. The massive rocks occur partly as dykes and intruded sills but mainly as undoubted *coulées*, and are described as various types of diabases and augite-porphyrites. Of these the former belong to the earlier eruptions, which were submarine, while the latter are found especially characterising the later eruptions, which were subaerial, the differences between the two sets of rocks being due to the different conditions under which they consolidated. It is also noticed that the diabases occur only in thick flows,

and graduate at the edge into andesitic types ("porphyrites," etc.), which also form the smaller flows. The diabases or dolerites have sometimes a granular, sometimes an ophitic structure, and only very exceptionally contain olivine. The felspar is sometimes labradorite, sometimes andesine, and in the latter case micropegmatite is also present. The augitic porphyrites show every gradation from holocrystalline to perfectly glassy rocks, and this in two parallel series, one of ordinary andesitic types, the other with a variety of radial, spherulitic and variolitic structures. The thoroughly glassy type, which in at least one flow retains its original character, is of great interest from the extreme rarity of such rocks, except as narrow selvages. The basic glass, often with scoriaceous and pumiceous structures, is also an important constituent in the fragmental volcanic accumulations associated with the lavas. These occur abundantly except in connection with the earliest outbreaks, in which the volcanic activity had not yet reached the explosive stage. There are subaerial tuffs, composed of more or less fine volcanic *débris*, enclosing spheroidal scoriaceous bombs of andesitic material and other fragments, and cemented by chlorite and other secondary products. Distinguished from these are stratified submarine tuffs, sometimes fossiliferous, or again containing pisolitic iron-ores not unlike those found in Antrim in similar circumstances. There are also beds of breccia, consisting of angular fragments of the porphyrites cemented by calcareous matter. Barrois clearly shows in this memoir how in the Menez-Hom district erosion has laid bare almost the whole apparatus of vulcanicity in those early times. The phenomena are in many respects comparable with those of the Carboniferous in the basin of the Firth of Forth as described by Sir A. Geikie (1879), except that "necks" marking the actual orifices of eruption have not yet been discovered in the Finistère region.

In America the recognition of the essential nature of the older volcanic rocks has been retarded by various circumstances. An immense development of such rocks, chiefly of pre-Cambrian age, exists along the eastern border of

North America and in other areas on that continent; but they have been, for the most part, so modified by dynamic and other metamorphosing agents, that superficially they differ widely as a whole from the fresh Tertiary lavas so magnificently displayed to the west of the Rocky Mountains. Their significance has thus been overlooked both by Wernerians, such as Sterry Hunt, and by the holders of certain extreme "metamorphic" theories, such as Dana and Logan, the one school regarding the schistose and foliated crystalline rocks as something *sui generis*, the other considering them as necessarily altered sediments. To this it must be added that petrology in America has drawn its inspiration largely from Germany, and not a few of the younger workers have been trained partly in the laboratories of Leipzig, Heidelberg, etc.

Nevertheless a number of American geologists, Selwyn, Wadsworth, R. D. Irving, G. H. Williams, Van Hise and others, have clearly recognised these ancient volcanic rocks as lavas and tuffs of various petrographical types, whose differences from corresponding products of Tertiary and Recent age result merely from the vicissitudes through which they have passed in their long life-history; and the description of the abundant material from this point of view has already been begun. The ancient volcanic rocks in America about which we have at present most information are situated in the country about Lake Superior and in what we may call, in a broad sense, the Appalachian region, occupying an enormous extent in the east of the United States and Canada.

In the Lake Superior region it has been recognised, chiefly owing to the work of Wadsworth, Pumpelly, Irving, Williams, and Van Hise, that volcanic rocks of various types play an important part in the constitution of some of the extensive pre-Cambrian formations there developed. Some of these rocks have undergone more or less profound alterations, and in the detailed study of them much still remains to be done.

As regards the eastern region, Diller gave in 1881 a somewhat detailed account of the petrographical characters

of the felsites and associated rocks of Marble Head in Massachusetts, which Wadsworth had recognised two years earlier as devitrified rhyolites and altered ashes ("porodites"). More recently some advance has been made in the study of the old volcanic formations of other tracts.

To G. H. Williams (12) we owe a clear account of some of the ancient lavas of the Appalachian region as developed at South Mountain in Pennsylvania and Maryland. The majority of the rocks are old rhyolites, and, in spite of secondary changes and frequent crushing, the author recognises not only the flow and banded structures, but spherulites, lithophyses, pumiceous modifications, and other well-known features of acid lavas. The rocks carry porphyritic quartz and an alkali-felspar, but rarely any ferromagnesian mineral. The ground-mass is a mosaic of quartz and felspar, but that this is often due to devitrification and recrystallisation is proved by the preservation in many instances of the characteristic structures of the acid glasses. Basalts are also found, retaining their original structures, ophitic, etc., and skeleton-crystals of olivine, but usually more weathered and more crushed than the rhyolites. In both types of lava epidote is one of the dominant secondary minerals, and in the rhyolites it is in great part the manganese-epidote, piedmontite. The lavas are accompanied by fine banded ashes, flow- and tuff-breccias, pumiceous bombs, and other concomitants of ordinary volcanic eruptions. The whole group underlies strata containing a Lower Cambrian (*Olenellus*) fauna.

In a later paper Williams (13) extends his observations, and describes typical devitrified obsidians, rhyolites, breccias, etc., from Maine in the north and from North Carolina in the south. From a general review of the literature of the subject, he shows the high probability that such ancient lavas and pyroclastic rocks will be found to occur throughout the whole region from Eastern Canada and Newfoundland to the Carolinas and Georgia. In numerous cases, although no detailed descriptions have been given, geologists have recognised the igneous, and sometimes the volcanic, nature of the rocks. In other cases there is reason to believe that

such terms as "metamorphosed slates," "siliceous slates," "ribbon jasper," "chert," etc., have been applied by field-geologists to wide extents of old volcanic rocks. Many of these are of pre-Cambrian age (Huronian, etc.), but others occur at various horizons among the Palæozoic strata. From so large a field it is to be anticipated that valuable information will be forthcoming in the near future.

Researches in various parts of America have also done something towards filling the gap in point of time between Palæozoic and Tertiary igneous rocks. Indeed even in Europe this gap seems to have been somewhat exaggerated. Both intrusive and volcanic rocks of Triassic age are known in the Tyrol, in the French Alps, etc. Others of Cretaceous age occur in Portugal, in Moravia, Silesia, Galicia, and the Russian province Wolhynia, and in the Crimea. Some of these are of peculiar characters; others have been described by Lagorio under such names as mesodacite, mesobasalt, etc., and differ in no essential from their younger equivalents. According to Reyer and others, forerunners of the Tertiary trachytic eruptions appeared in the Euganean area as far back as the Upper Jurassic period. Such facts have, of course, been appreciated by many continental geologists, and when such influential leaders as Rosenbusch consent to abandon the age-criterion in the classification of rocks and revise their petrographical nomenclature accordingly, a serious obstacle to the progress of petrology will have been removed.

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ALFRED HARKER.

THE MEASUREMENT OF TEMPERATURE.

IN these days of specialisation the methods of investigators differ as widely as their aims, and apparently they have little in common except the desire to do their utmost for the advancement of science. Let us suppose an unscientific but intelligent visitor to pass, say, from a physical to a chemical, and thence to a biological laboratory. Except that in each are many instruments built up of glass and metal their contents would present but little similarity, nor, with one exception, would repetitions of the same apparatus be noticeable. One instrument, however, would be common to all, would be found in every room, possibly on every table, and inquiry as to its name would, I need scarcely say, receive the reply: "A mercury thermometer". Our visitor would be led to the natural conclusion that such an essential instrument to which so much attention must have been devoted would have arrived at a high degree of perfection and that its history would be one of progress and evolution.

Such being the case it is strange to reflect that the mercury thermometer has remained practically unchanged from the time (1714) when Fahrenheit first suggested the use of certain fixed points to the present day.[1] True, Cavendish in 1780 investigated the conditions under which observations with this instrument must be made, such as the effect of the immersion of the stem, etc.,[2] and in recent times the labours of Regnault, Rowland, Crafts, Pickering and Guillaume have added much to our knowledge of its imperfections; although, with the exception of the last named, they have done but little to help us to remedy them.

[1] The origin of the Fahrenheit fixed points is so little known that I may be excused a reference to it. His lowest "natural cold" was that of his freezing mixture, his "highest natural heat" that of the human body. Thus the freezing and boiling points of water are, on that scale, consequent, not original ones (see Gamgee, *Cam. Phil. Soc. Proc.*).

[2] Thorpe, *Essays on Historical Chemistry*, p. 75.

SECTION I.

Errors arising from faulty determination of temperature are, I believe, much more common than is usually supposed to be the case, and in an earlier number of this review[1] I have called attention to some of the lamentable consequences arising from such errors. True, the labour involved in the complete standardisation of a mercury thermometer is appalling. In addition to the necessary observations of the accuracy of its "fixed points" it includes :—

(1) A calibration of the bore.

(2) The determination of the temporary changes of zero and repeated observation of its permanent rise.

(3) The estimation of differences caused by movements from the horizontal to the vertical position.

(4) Observation of the effect of changes in the external pressure.

(5) Determination of the differences resulting from the rate of rise of temperature.[2]

(6) The application of the correction (at best but approximate) for any unimmersed portion of the stem.

(7) The reduction from the mercury in glass scale to the air scale.

No man whose time is valuable can, as a preliminary to some investigation, afford to spend months in the determination of such quantities, or, having done so, contemplate unmoved the fact that a careless motion of the hand may render all his labour of no avail. Let us suppose, however, that all the above quantities have been successfully ascertained, the labours of the observer are by no means ended, for one determination of temperature is of itself a lengthy operation. I can best illustrate this by an extract from my note-book, of the observations and corrections in a single case (Tonnelot's hard glass thermometer, No. 11,048).

[1] April, 1894.

[2] No accurate determination can be made by a mercury thermometer whose temperature is falling (see *Phil. Trans.*, 184 A, p. 442).

OBSERVATIONS.

A	Reading (stem vertical and all at one temp.)	$25{\cdot}615°$
	Barometer (corrected)	$754{\cdot}8$ m.m.
	Distance from centre of bulb to surface of water	400 m m.

The thermometer was then placed in melting ice, and watched until the *lowest* point touched by the column was ascertained.

B	Reading (stem vertical)	$0{\cdot}051°$
	Barometer (corrected)	$754{\cdot}8$ m.m.
	Distance from centre of bulb to surface of water	66 m.m.

CORRECTIONS.

A	External pressure $= 754{\cdot}8 + \frac{400}{13{\cdot}6}$ . .	$= 784{\cdot}2$ m.m.
	Hence external pressure correction .	$= -0{\cdot}003°$
	Internal pressure correction . . .	$= +0{\cdot}041°$
	Calibration correction	$= +0{\cdot}004°$
	sum	$= +0{\cdot}042°$
B	External pressure $= 754{\cdot}8 + \frac{66}{13{\cdot}6}$ . .	$= 759{\cdot}7$ m.m.
	Hence external pressure correction .	$= 0{\cdot}000°$
	Observed zero depression . . .	$= -0{\cdot}051°$
	Calibration correction	$= 0{\cdot}000°$
	Internal pressure correction . . .	$= +0{\cdot}008°$
	Hence true zero depression	$= -0{\cdot}043°$
	Hence sum of corrections from A and B	$= +0{\cdot}085$
	Hence corrected reading $= 25{\cdot}615° + 0{\cdot}085$	$= 25{\cdot}700°$
	Fundamental error	$= -0{\cdot}011°$
∴	Temperature on true mercury scale	$= 25{\cdot}689°$
	Correction to hydrogen scale . . .	$= -0{\cdot}096°$
	True temperature on hydrogen scale .	$= 25{\cdot}593°$

Had the temperature not been steady, a further correction depending on the rate of rise would have had to be included.

Although in the above example many of the corrections are of opposite signs and the total effect small, this is not

necessarily the case. Occasionally all the signs are similar, and the effect is considerable, amounting in some cases that I have observed to nearly 0·4°C. Again, the corrections in the case of hard glass thermometers are smaller than when soft glass is used, and nearly all the thermometers used in our English laboratories are of the latter material.

It is therefore obvious that the endeavour to obtain one accurate reading by means of a mercury thermometer involves considerable labour, and presupposes a prolonged and exhaustive study of the thermometer used. Many observers disregard these precautions and defend themselves by the following statements: (1) that certain of these corrections are uncertain and possibly imaginary; (2) that cases requiring such extreme accuracy are rare. The opinion of such an authority as Dr. Guillaume ought alone to be a sufficient reply to No. 1. It is, however, an objection which I confess to having at one time entertained, and I find it is very prevalent. The best answer that I can make is to give particulars of a case which has come under my notice within the last few weeks.

In February, 1893, I received from the International Bureau of Weights and Measures the thermometer by Tonnelot, No. 11,048, and in *Phil. Trans.*, A, vol. clxxxiv., pp. 427-433, I have already published a full account of the comparison of this thermometer with my own standards. In the autumn of last year I received from Professor Threlfall of Sydney University a request to procure for him, if possible, thermometers fulfilling certain specified conditions. Three thermometers were accordingly prepared by Tonnelot, standardised at the International Bureau and they reached my hands the first week of June, 1894. One of these was for low temperature, and I have made no comparisons between it and others. No. 11,402 had a range from − 1° to + 21°C. No. 11,403 had a range from − 1° to + 1° then a bulb and then from 20° to 41°C. Each degree on these thermometers was divided into twenty parts.

On June 8th and 9th Mr. C. T. Heycock was so good as to assist me in a careful comparison of these instruments

with No. 11,048 and with some other thermometers. I had but one suitable micrometer scale, and this was used when observing No. 11,048, which was divided to $\frac{1}{10}$th of a degree. The probable limit of observational error in this case was therefore only 0·001°C. The other thermometers were observed through ordinary reading telescopes, and as it was impossible to estimate fractions of the spaces less than $\frac{1}{10}$, the probable limit of error of a single observation was 0·005°C. The actual *uncorrected* readings differed by as much as 0·4°C. in certain cases. The *corrected* results of the separate readings differed in no case by more than 0·010°C. and only in one case by more than 0·006°C., which is about the probable observational error. The mean result of the whole of the thirteen comparisons made between 11,402 and my standard (taken at regular intervals from 12° to 21°C.) gives an absolutely identical result of 17·1417!

Only four comparisons were made between 11,403 and my standard, the means being 25·197 and 25·200 respectively. The exact correspondence in the former case must be in part fortuitous, but results such as those are a wonderful testimony to the accuracy of the tables of corrections supplied by the Bureau, and an examination of the full table of numbers obtained by us during the above comparisons would, I think, convince the most sceptical of the validity and necessity of the various corrections.

As regards objection No. 2, if accuracy of such an order is possible there can be no doubt of its desirability. For example, the perfection of our methods of electrical measurements is now so great that errors amounting to 0·01°C. in the estimation of the temperature of Clark cells and of resistance coils, etc., become serious, and all efforts to trace, by means of a mercury thermometer, the variation in such quantities as the specific heat of water and other substances with change of temperature, are hopeless, unless the previous standardisation of the thermometer has been satisfactorily accomplished.

I do not wish in any way to disparage the standardisations performed at Kew, but it is evident that they do not

meet, and I conclude are not intended to meet, the demands of exact inquirers. Very wisely they, in no case, give the correction term beyond 0·01°C., for they do not supply any of the data (as far as my knowledge goes) for making the corrections rendered necessary by change in position, in external pressure, in the temporary change of zero consequent on the preceding temperature of the thermometer, etc., and as the combined effect of such corrections alone may considerably exceed 0·01°C. it is evident that until the department supplies further information it has carried its method of correction to its limits. I am afraid that the issuing of those Kew certificates, which give the second decimal figure of the correction, so far from being a prevention is a cause of inaccuracy, as, unless used with a full knowledge of the variations consequent on changes in the conditions, they impart a false confidence to their possessors.

In this section I have endeavoured to show that although it is *possible* to accurately determine ordinary temperatures by means of a mercury thermometer, the process is an intricate and unsatisfactory one at the best, and that even under the most favourable circumstances, the determination of a *single* temperature requires considerable expenditure of time and attention. The removal of the thermometer from its initial position, its immediate immersion in ice, the complete immersion of the stem, the measurement of the external pressure, etc., make up a series of operations which the conditions of an experiment usually render difficult, if not impossible.

SECTION II.

A consideration of the difficulties inseparable from the use of mercury thermometers would naturally lead to the conclusion that if some more simple but accurate form of thermometer, free from such unavoidable causes of error, could be devised, it would meet with general acceptance. The platinum thermometer, as now constructed, is an almost ideal instrument, and is especially free from those defects inseparable from mercury thermometers. It can be used under circumstances which forbid the employment of a mercury one, and it surpasses that instrument both in accuracy and in

range. It is difficult to account for the fact that such an instrument remains comparatively neglected, and I am glad of this opportunity of bringing its claims under the notice of so large a number of scientific workers. I propose to briefly review the evidence we now possess concerning the accuracy of this method of determining temperatures.

The bibliography of this subject is small, and at the end of this article will be found what I believe is a fairly complete table of such communications as have been published. I shall refer to these papers by the figures allotted to them in that list.

I do not propose to enter into any details as to the method of making or using these instruments as full information on such points will be found in papers (7), (8) and (9); for my object is rather to present the evidence in favour of their accuracy in such a manner that its cumulative weight may be appreciated. It may be advisable to first clear the ground by disposing of some objections which I find are commonly entertained by (I may remark) those who have had no experience in the use of the instrument.

1. The general scepticism as regards the constancy of the platinum thermometer may, I think, be traced to the adverse report delivered by the influential committee appointed by the British Association in 1873 (1). In 1874 they reported that the indications of the instruments examined changed by as much as 50°C. after heating in a common furnace, and also that the fixed points altered by mere lapse of time. It must, however, be remembered that this committee was appointed to examine, and did examine, but one form of apparatus, *viz.*, the Siemens pyrometer, and their adverse conclusion should in no way affect the reputation of the modern form of instrument.

In Siemens's pyrometer a platinum wire was wound on common clay, placed in an iron tube, and exposed to a high temperature. Changes exceeding those found by the committee would be anticipated by observers who have studied the behaviour of platinum wires under such circumstances, and my own feeling is one of surprise that the

alterations were so small! If constancy is to be secured the protection from the furnace gases must be complete, and the material on which the wire is wound selected with great care. It is a significant fact that the committee pointed out that the resistance of the coil which was placed in a platinum, instead of iron, sheath remained practically unchanged, and it is probable that what small change was noticed therein was a consequence of the insufficient annealing of the wire. Again, the relation between the resistance and the temperature of the wire does not appear to have been determined by Sir William Siemens with sufficient accuracy. The formula suggested by him, $R = \alpha T^{\frac{1}{2}} + \beta T + \gamma$, is deduced from insufficient data.

I extract the following from Callendar's paper ((9), p. 110): "It appears from Sir William Siemens's account of his experiments that they were undertaken rather with a view of graduating a commercial pyrometer than of investigating the law of change of electrical resistance. Temperatures up to 350°C. were determined *by mercury thermometers* in an air- or oil-bath, and it does not appear that any corrections were applied to their readings. The individual observations are somewhat irregular and often show divergencies amounting to two per cent. and over. Only three observations at higher temperatures are given; they show a mean deviation of about 30°C. A copper ball-pyrometer was used to determine the temperatures, which are given as 810°, 835° and 854°C.; the corresponding temperatures deduced by Siemens's formula from the observed resistances of the platinum pyrometer were 772°, 811° and 882°C. . . . The resistances apparently were only measured to about one per cent. in most cases, and the temperatures are only given to the nearest degree."

2. Another objection occasionally urged is the change of resistance found in platinum filaments when used in incandescent lamps. A little reflection, however, will show that the conditions are in no way similar. Callendar observes on this point ((9), p. 105): "The sudden heating and cooling of the wire when the current is turned on or off, and the intense radiation which keeps the surface at a lower

temperature than the central portions, must be a severe strain on the wire. It is also evident that any crack or flaw in the surface will tend to be intensified by the local development of greater heat, and if the wire is heated to a temperature near its melting point where it begins to be appreciably volatile, this action must inevitably produce serious results. If a wire which has been thus treated be examined under the microscope its surface will generally be found to be cracked and scored in a manner which is in itself amply sufficient to account for the increased resistance and brittleness."

With the exception of objections based on the apparent complexity of the instrument (which I deal with later) the above are the only serious adverse criticisms which it is necessary to notice.

Before adducing the evidence on the other side, it would be as well to briefly explain the terminology which has been found to be convenient. Callendar in 1887 ((3), p. 163) suggested the use of the term "platinum temperature" to denote the reading on a scale so constructed that a rise of 1° of that scale would at any temperature increase the resistance of a platinum wire by $\frac{1}{100}$ of the difference between its resistance at 100° and at 0°C. Hence if R is the resistance of a platinum wire at a temperature t (measured in degrees Centigrade by the air thermometer), and if R_1 and R_0 are the resistances of the same wire at 100°C. and 0°C. respectively, and if pt stand for the platinum temperature, then

$$pt = \frac{R - R_0}{R_1 - R_0} \times 100.$$

In the same paper Callendar communicated to the Royal Society the results of an exhaustive series of comparisons of the resistance of a platinum wire and the temperature as indicated by the air thermometer in whose bulb it was wound. No one can, I think, rise from the study of this important communication without admitting that the experimental evidence produced was sufficient to establish the following conclusions (p. 161, *ibid.*):—

(*a*) The self-consistency of the platinum thermometer

has been abundantly verified by the experiments. If the wire is pure to start with, its resistance is always the same at the same temperature.

(*b*) The relation between the platinum and the air temperature is closely represented by the parabola—

$$t - pt = \delta\left\{\left(\frac{t}{100}\right)^2 - \frac{t}{100}\right\} \quad . \quad . \quad . \quad (d).$$

The experimental evidence given in support of (*b*) would be sufficient to establish the necessary relation provided we could make certain that all platinum wires used should be of the same degree of purity as that used by Callendar. It appeared probable, however, that a greater or less degree of purity might entirely alter the character of the curve.

In the autumn of 1889 I commenced a series of determinations of certain freezing and boiling points by means of platinum thermometers. At that time I had not had the advantage of reading Callendar's paper, nor was it brought under my notice until my observations were nearly completed; the inquiry was therefore conducted on independent lines. In order to standardise my instruments I had assumed the following "fixed points" in addition to those given by melting ice and steam at 760 m.m. :—

B. P. of	Naphthalene at 760 m.m.		= 218·06	Crafts
,,	Benzophenone	,,	= 306·08	Crafts
,,	Sulphur	,,	= 448·38	Regnault.

I standardised eight thermometers by means of the above five points. These thermometers were of different patterns, their coils were formed of different specimens of platinum wire differently insulated, although the majority of the coils were wound on calcined asbestos and enclosed in hard glass tubes. The conclusions I arrived at may be summarised as follows ((7), p. 64):—

(*a*) The readings of each thermometer are constant when the temperature is the same.

(*b*) Different thermometers whose coils are formed of different specimens of platinum do *not* give the same platinum temperature when at the same actual temperature.

It will thus be seen that my independent investigation agreed as to (*a*) with the conclusion arrived at by Callendar, and was in direct contradiction to the report of the British Association Committee with regard to the Siemens pyrometer in which the wire was insufficiently protected. Conclusion (*b*) appeared discouraging, as it involved a separate and difficult standardisation of each thermometer before use.

On comparing my results with Callendar's it was evident that the upper portions of my $t - pt$ curves departed considerably from the parabolic form, and we, therefore, appeared to differ in our conclusions. The difference was so marked that I consulted with him, and we decided to make a thermometer similar to mine out of a portion of his original coil (which he had fortunately preserved) and expose it to sulphur vapour under the same conditions as those prevailing during my own experiments. The result, assuming formula (*d*) and Callendar's previously found value of δ, gave the boiling point of sulphur as 442·3°C., nearly 6°C. below Regnault's number. It was therefore evident that either Callendar's value of δ was wrong, or that the thermometer in my apparatus did not attain the temperature of sulphur vapour, or else that Regnault's value of the boiling point of sulphur was too high. The matter appeared so important that the summer of 1891 was devoted by us to its investigation, and an account of the work will be found in (8). We believe that we then established the following points: (*a*) that Callendar's value of δ, as obtained during his experiments in 1887, was practically correct; (*b*) that the bulb of the thermometer in my apparatus did not attain the temperature of the sulphur vapour unless suitably screened and that the error due to this cause might amount to 1·05°C.; (*c*) that Regnault's value of the boiling point of sulphur was too high, the results by our air thermometer determinations being 444·5°C. as against Regnault's 448·4°C.

The curves obtained by me in 1890 were now re-drawn, substituting *our* value of the boiling point of sulphur for Regnault's. A study of those curves then led to the following important conclusions:—

(*a*) That although the values of δ varied greatly, the curves remained practically parabolas.

(*b*) That the assumption of the parabolic form and of the respective values of δ obtained by observations in sulphur vapour gave values for the boiling points of mercury, benzophenone and naphthalene, which were practically the same whatever the sample of the wire used, provided it was of ordinary commercial purity.

(*c*) That the boiling points of benzophenone and naphthalene, as deduced in the above manner from the numbers obtained from my experiments of 1890, agreed closely with the values given by Crafts; for example—

	Griffiths	Crafts
Naphthalene . .	217·94°C.	218·06°C.
Benzophenone . .	305·82°C.	306·08°C.

and to appreciate the value of the results it must be remembered that they depend entirely on the validity of the formula (*d*) and on the correctness of the boiling point of sulphur as determined by us in 1891. A reference to the original paper will show that these conclusions were borne out by determinations of the boiling points of methylsalicylate, triphenyl-methane, mercury and the freezing points of Sn, Bi, Cd, Pb and Zn. True, the determinations of the air temperature of these points by previous observers differ very greatly from each other, and none appear to have been determined with the care and accuracy which distinguished the work of Crafts; but the importance of the comparison lies in the fact that platinum thermometers, whose constants differed greatly, gave almost identical values of these temperatures, which ranged from 184° to 445°C. As it was possible that the departure of the $t - pt$ curve from the parabolic form might become marked at ordinary temperatures, an elaborate comparison was made between a platinum and an air thermometer at every 5° from 0° to 100°C. A table of the results will be found in paper (8) (p. 155). This series of experiments led to the same conclusions as the observations at higher temperatures.

It thus appears that a complete standardisation of a platinum thermometer can be made by observation of its resistance at three temperatures only, and the ones selected by us for the purpose are 0°, 100° and 444·5°C. (sulphur vapour at 760 m.m.). Great care must, however, be taken that the thermometer, when in sulphur vapour, is thoroughly screened as described by us ((8), p. 143). Our experiments with various specimens of sulphur indicate that the sample used need not be of extreme purity, the ordinary impurities not affecting the temperature of the vapour when the boiling has been continued for some time. Thus R_s, R_1 and R_0 being known pt_s can be obtained, and hence the value of δ for the particular sample of wire used can at once be deduced from formula (*d*). The standardisation can now be regarded as complete, and we have an instrument whose fixed points do not change, provided that the wire has been carefully annealed, no matter what temperature it has been previously exposed to, and whose readings are independent of position and external pressure. Its capacity for heat is small, it can be made of almost any size, so as to give the mean temperature of a space, or the temperature "at a point," it can be placed interior of any apparatus and read at any convenient distance. Also, if, as should invariably be the case, the stem is supplied with double electrodes, the readings are uninfluenced by the temperature of the stem or leads. I derived such confidence from the experiments performed with platinum thermometers, whose constants had been determined in the manner I have described, that I standardised the mercury ones used during my determination of the mechanical equivalent of heat by platinum thermometers only. At the close of that investigation, those mercury thermometers were carefully compared with the standard supplied by the International Bureau. The results of the comparison are given in detail ((11), p. 430) and show that in actual elevation a difference of 0·005°C. was found but that the value of temperature ranges was practically identical.

Again, a further comparison was made by Callendar and myself during the summer of 1893, between a new form of air thermometer, whose indications were independent of

changes in the external pressure[1] and the Paris, and some platinum standards. A summary of the results will be found in paper (13), and they but more firmly establish the conclusions previously arrived at.

There are indications that the formula (*d*) gives the relation between the air and platinum scales at very low temperatures. In a communication to the *Phil. Mag.* (10) it was pointed out that we can find at what platinum temperature the resistance of platinum will vanish by placing o for R in the formula

$$pt = \frac{(R - R_0)}{R_1 - R_0} = 100$$

and then by using the value of δ peculiar to the wire and assuming the parabolic formula to hold over so large a range, we can deduce the corresponding value of t. The method was applied to all those thermometers whose constants had previously been published and the mean result gave $-273{\cdot}9^\circ$C. The experiments of Professors Dewar and Fleming (12) led them to the conclusion that the electrical resistance of platinum wire would vanish at absolute zero, and thus we have what seems to me very strong evidence of the accuracy of the (*d*) formula at low temperature.[2]

As regards temperatures exceeding 700°C. there is no reason to suppose that the departure of the $t - pt$ curve from the parabola becomes important. It is difficult to carry the comparison with the air thermometer above the temperature already obtained. What we require for high temperatures, however, is not so much the measurement on the air scale, but some practical standard to which they may be referred, and even if the relation between t and pt at

[1] Callendar, *Roy. Soc. Proc.*, January, 1891.

[2] In cases where a high order of accuracy is not a necessity, a yet simpler mode of graduation is thus suggested. Having found the value of R_1 and R_0 if we assume $R = 0$ when $t = -273{\cdot}7^\circ$C. we can obtain an approximate value of δ and thus dispense with the observations in sulphur vapour. The results obtainable by this simple method are but approximate. In seven thermometers whose constants were given in paper (10) the greatest error caused by the adoption of this method amounted at 150° to ·44°C., but if the thermometers are intended for use between 0° and 100°C., the probable error would not exceed ·05°C.

such temperatures differs considerably from the results obtained by exterpolation the platinum thermometer yet supplies a standard to which all such measurements can be referred. Thus, as far as regards range, the advantages are entirely on the side of the platinum thermometer. It is true that mercury thermometers can, with proper precautions, be used over a very considerable range of temperature, especially those excellent instruments constructed by Niehls of Berlin, which contain gas at a high pressure. Their great drawback, as far as accuracy is concerned, is the difficulty with regard to the stem temperature, a matter of great importance in thermometers of this description. The circumstances under which they are used almost invariably prevent the complete immersion ; thus an estimation of the mean stem temperature is a matter of great difficulty, and however accurately they may have been graduated by the makers, it is hopeless to expect accuracy of a higher order than 1° or 2° C. The same remarks apply to the potassium-sodium alloy thermometers, and the latter I find are in addition very subject to changes in zero after exposure to high temperatures.

The cumulative weight of the evidence I have summarised in this section is, I think, great, and it is discouraging to find that so little use has as yet been made of it in the scientific world. In many cases manufacturers have shown themselves ready to take advantage of so simple a method of estimating high temperatures, but a platinum thermometer is, as yet, rarely to be found in a physical laboratory. True, Messrs. Heycock and Neville have applied the method with complete success during their investigations into the melting points of alloys (5), and Professors Dewar and Fleming have used it to a certain extent in their determinations of low temperatures (12), although the last-named observers evidently distrusted the relation given by the (*d*) formula and have stated their results in the platinum temperature-scale. Such examples of its application, however, are rare, and in no case (except in Callendar's work and my own) do I find any record of its use for the *accurate* determination

of ordinary temperatures, for which purpose I consider it well suited.

With regard to the difficulties of making observations with this instrument, those who urge this point evidently suppose that in order to obtain a temperature by a mercury thermometer it is merely necessary to observe the position of the top of the mercury column, and if they desire no greater accuracy than is obtainable without further exertion I admit that simplicity *is* on the side of the mercury thermometer. Where accuracy is required I think that I have shown in Section I. that the use of the mercury thermometer is by no means simple, and that when we bear in mind the labour involved in the previous processes simplicity is all on the side of the platinum thermometer.

As to the difficulty of constructing the instrument, I am not aware that observers usually manufacture their own mercury thermometers, and since platinum thermometers are now supplied commercially the objection bears but little weight.

In conclusion, I would again quote from Callendar's paper (9): "I quite admit that it requires some special skill and experience to *make* a good thermometer, but the rest of the apparatus required is obtainable in almost any laboratory and it is easy to take the readings quickly and accurately after a little practice. The great superiority of the platinum thermometer in range, accuracy and durability will be found in the end to save as much time and expense as will more than compensate for the small trouble of learning to use it."

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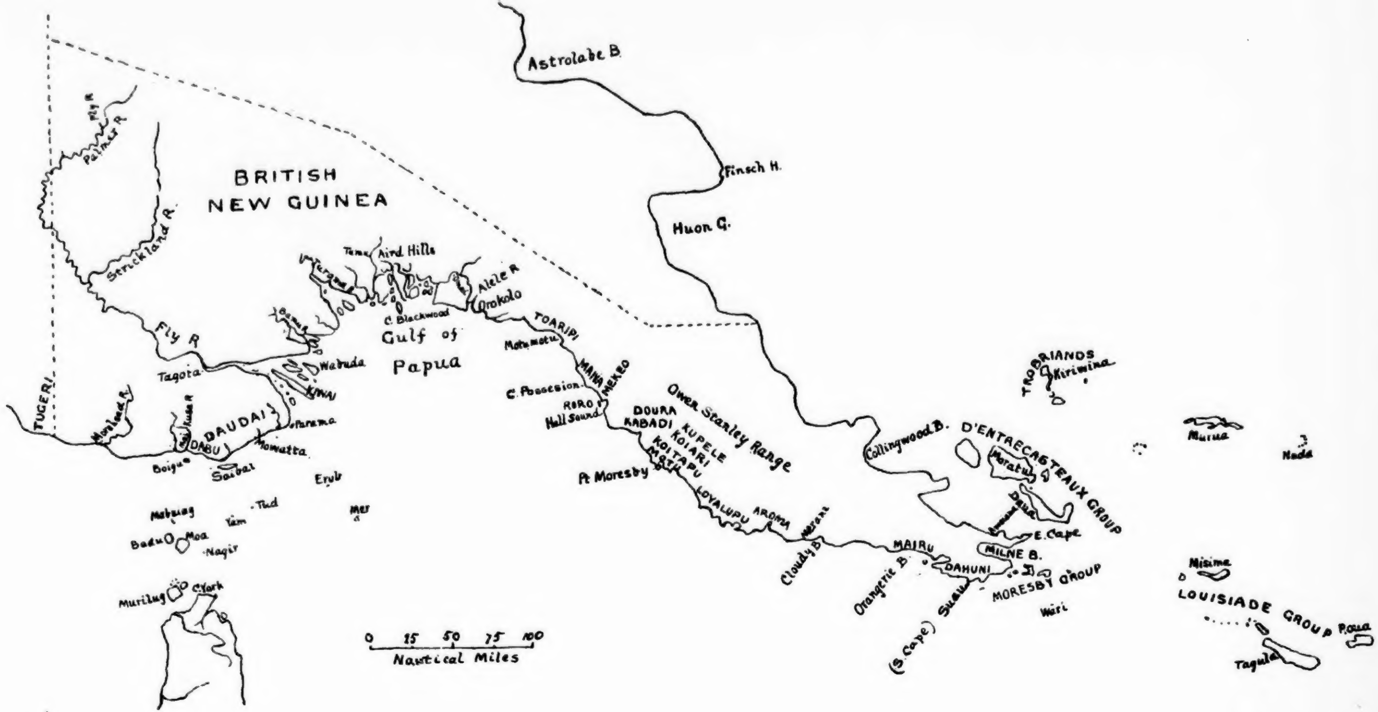

BRITISH
NEW GUINEA
Astrolabe B.
Finsch H.
Huon G.
Strickland R.
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DAUDAI
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Mowatta
Parama
Wabuda
KIWAI
Aird Hills
C. Blackwood
Gulf of
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Alele R.
Orokolo
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Hall Sound
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KOITAPU
MOTU
Owen Stanley Range
Pt. Moresby
LOYALUPU
AROMA
Cloudy B.
MAIRU
Orangerie B.
DAHUNI
(S. Cape)
MILNE B.
E. Cape
Dawa
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Collingwood B.
D'ENTRECASTEAUX GROUP
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Murua
Nada
Misima
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